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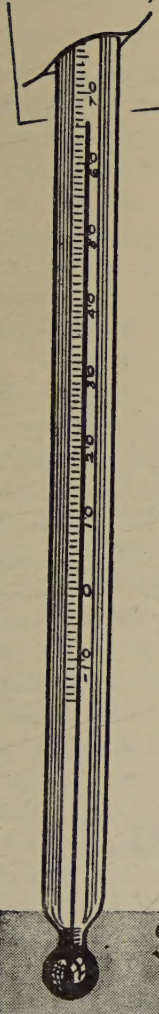

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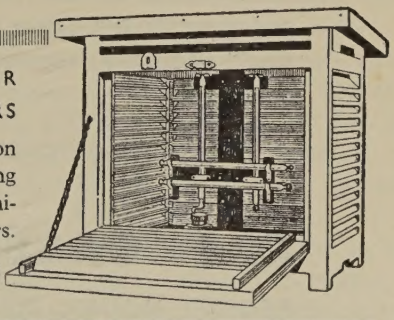

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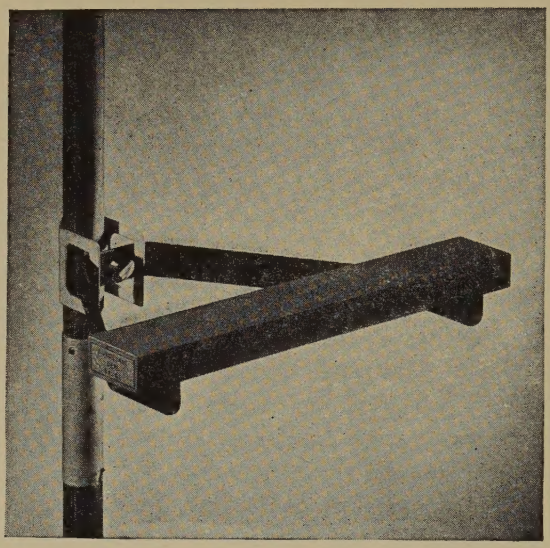

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REVIEWS OF MODERN METEOROLOGY—9

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Climatic variation

By GORDON MANLEY

Bedford College, University of London

Introduction

For a very long time the likelihood of climatic variation, its character, direction and possible extent have been subjects of acute interest to man. Deep in the ancestral consciousness of Europeans are found the stories of past Golden Ages, of idyllic realms where the weather could be relied on; deep, too, is our fear of extremes, of the parching drought of the Sahara, or, behind the Alps, the overmastering cold of the great Hercynian forest and the endless unknown beyond. The legend of Prometheus enshrines our recognition of the debt we owe to our technicians, whose accomplishment through the centuries has enabled civilisation increasingly to flower in undreamt-of latitudes and climates. Technical accomplishment has ensured that European universities have been transplanted to Harvard and Lima, Edmonton and Singapore, Arizona and Natal.

Yet the fundamental comfort, prosperity and progress of large areas of the earth's surface depend on the amount of spare energy available; and the possible tax on energy imposed by climatic vicissitudes must remain one of our greatest anxieties only to be overcome by large-scale capital investment. The building of power-stations in Norway is the modern equivalent of the extensive glazing of windows four centuries ago. Coping with day-to-day changes of weather is now largely a matter for individual effort and decision; coping with seasonal changes in respect of water supply or grain shipment is a matter for communities; we are just beginning to confront the need to insure against longer-term variations which may in time require action on an international scale. For long this has been evident with regard to the problems of development and of enhanced security in warmer lands on the desert margin; studies of the vicissitudes of the Indian monsoon, of Australian rainfall, of Arizona rainfall and its association with tree-rings may be recalled. Problems of the desert margin were indeed a matter of concern even in classical times. But man's opportunities are linked to the cold tree-line as well as the dry tree-line.

We are now approaching a stage in which the variations in temperature in more northern lands will assume enormous potential importance. Suffice it to refer to the length of the operating season for the St. Lawrence seaway; to the yield of grain and coal in the Pechora basin; to the fisheries of the Arctic-Atlantic and even the water power so extensively being harnessed in the High Alps. Moreover the possible variations in the agricultural yield of the greatest consumer of imported foodstuffs – Western Europe – are likely to be world-wide in their effects. The brake on the circulation imposed by American tariffs is far less significant than that which might accrue from a more persistent Scandinavian or Hudson's Bay high.

For these reasons the study of climatic variation is of quite fundamental importance; yet in common with other fundamental studies it is beginning to assume a fine structure of remarkable complexity. This is because of the extraordinarily broad scale of the field, both in space and time, in which it is needful to become conversant with the evidence. For many reasons the study has received marked impetus in recent years and is beginning to provide a variant approach to some of the basic problems of meteorology, as Simpson (1934), Angström (1939), Willett (1949b), Flohn 1952 and others have shown. The field is being actively cultivated and in the next few years marked advances can be expected; a recent review has been given by Estienne (1952).

The present as the key to the past

We may remind ourselves that, since the total amount of air is fixed, and has probably remained so since an early stage in geological time, the average barometric pressure over the whole surface of the earth must be always the same, and an excess in one region must be compensated by a deficit in some other. It follows that if for some reason such as increased absorption of solar radiation in one part of the earth's surface we lower the atmospheric pressure, then it will have to be higher somewhere else; and we may therefore expect an enhancement of pressure gradients and of wind speeds. Carrying the argument forward, increasing the solar radiation should lead to increased briskness or vigour of the atmospheric circulation taken over the whole earth.

On the other hand, following Simpson's well-known argument, in cloudy temperate lands an increased cloudiness would result; increased reflection of solar radiation from the clouds would lead to regional cooling; as a consequence, an increase in the general vigour of the atmospheric circulation would be more likely to lead to departures from the initial pattern than to conformity. It follows therefore that throughout the earth's history we should expect to find considerable year-to-year variations in both the vigour and the pattern of the atmospheric circulation. Moreover, we are not yet able to say whether solar radiation varies in the manner that some have been disposed to claim; we need to know a great deal more about the composition of the atmosphere and its varying capacity for transmission, absorption and reflection of radiation. This again is a fundamental problem already considered in an earlier contribution to this series (Goody and Robinson 1951). One way by which the climatologist can contribute to this problem is through mapping the results.

Secondly, the earth's surface qualities have varied considerably throughout geological time; in smaller measure they do so today, most obviously in the boreal continents with the alternation of the seasons. The forecaster engaged in the local problem of the allowance to make for night cooling over the sands of a tidal estuary is studying on a small scale the problem faced by the climatologist who is arguing about the effects of variations in the extent of land and sea; for which the geologist, with characteristic and commendable caution, may be ready to provide a certain amount of evidence. The forecaster on a November day who considers whether a low moving up the Baltic will occlude more or less rapidly on account of a newly-established snow cover in Lithuania is probably touching on the whole problem of the alimentation of the Pleistocene ice caps. But the time scale is very different. Yet it is the present writer's conviction that in the present can be sought the key to the past; the day-to-day synoptician will do well to integrate some of his findings into a greater pattern. Correspondingly, the student of climatic variation may be able to provide another answer to the physicist who is attacking the problem of the general circulation. For if the evidence can be properly assembled, for example that of the amplitude of temperature variation at the Equator during the Pleistocene Ice Age, we shall have a better idea of the extent and character of these braking effects on the

circulation which may be imposed as a result of changes in the character of the terrestrial surface.

These effects are of two kinds. In temperate latitudes, a great extension of the area covered by snow and ice leads to the further development of shallow cold anticyclones, on the flank of which, surface currents a few thousand feet deep flow more or less in opposition to the freer circulation of the upper westerlies and impose a brake, of thermal origin, on the eastward movement of surface disturbances.

A more direct mechanical impediment is from time to time provided by high mountain barriers. It is a very useful exercise indeed for anyone possessing some familiarity with synoptic charts to consider what would happen to the climate of Canada in the absence of a Rocky Mountain barrier, i.e., to compare the effects of mechanical and thermal brakes. That an extensive winter snow-cover would exist is undeniable and it might well be found to prevail to a greater depth than at present; in that event, would not the climate of the Prairie Provinces be worsened ? Such discussions beg the question whether the growth of mountain barriers, considered by some to be a significant factor in the development of the last glaciation, has necessarily the same effect in every latitude.

There is much room for discussion of such problems, as we are still very far from a knowledge of the relationship between the evolution of mountain systems, the extent of land and sea, and the climate throughout long stretches of geological time. Broad outlines can be given, such as that by Umbgrove (1947), who with others has not hesitated to suggest that there must be an association between the great orogenic episodes in the earth's geological history and the subsequent development of ice ages. We must on the other hand recognize the several very different time scales involved when we are tackling the many problems of climatic variation. While we have a general idea of the location of the peaks and hollows in the climatic record we are quite ignorant of the rates of change represented by the slopes between. We do not know, in other words, whether the major climatic variations are capable of occurring in a relatively jerky manner or not. We are only slowly approaching the stage in which we can say at what rate minor variations in the pattern, sufficiently marked to provide recognisable evidence, are likely to develop. This applies on every scale. Hence studies of the small-scale fluctuations of the last two centuries may quite possibly provide the answer to the problems of larger order.

Before we can adequately discuss the reasons for climatic variations we must have, in effect, a more accurate map. We must be able to chart the course of the variations both in space and time. Stimulating mental exercise and speculation, like that of the scientists of ancient Greece, may well result from relatively scanty evidence, and this will for long be true with regard to variations of climate, inasmuch as we have the whole earth's surface and two thousand million years to cover before we can fully assemble the data. This space-time problem can however be attacked on five very different levels, with differing types of evidence and with diminishing precision.

The model provided by a turbulent river

We may regard the flux in time of the pattern of climatic events at any given place as analogous to the flow of a large and turbulent glacier-fed river. Many who have stood on a bridge over such a river will recall the results of observation. With very considerable frequency, trains of waves of tolerably regular length and amplitude develop, then disappear while others then break out in some other part of the stream, dissipating with equal rapidity. Nevertheless the highest crests and lowest troughs to be observed over any interval of time such as an hour can be estimated quite closely.

As the day wears on and the volume of the stream increases with the melting due to warmth above, the height of the highest crests can also be expected to increase together

with the mean level. But if the mean level increases sufficiently, i.e., the depth of the stream above the rocky bed, the wavy corrugations of the surface may gradually lengthen and diminish in amplitude. In other words, if we increase the energy of the stream and diminish the braking effect of the bottom, we diminish the amplitude of the irregular surface waves. If we put more obstacles on the bottom and thus increase the braking, the level remaining the same, we shall increase the amplitude of the irregular surface waves. Lastly, if we take the whole stream into a smooth channel and gauge it, we shall find that the surface level rises and falls with a curve dependent, in the instance cited, on the input of heat above. The analogue of this stream-gauging in regard to climatic events is to be seen in the smoothing process inherent in the adoption of running means.

TABLE 1. Orders of climatic variation

	Time-scale unit	
(1) Minor fluctuations within the instrumental record	10 years	Minor fluctuations which give the impression of operating over intervals of the order of 25-100 years, with somewhat irregular length and amplitude
(2) Post-Glacial and historic	10^3 years	Variations over intervals of the order of 250-1,000 years, e.g., the sub-Atlantic recession and others affecting vegetation in Europe and N. America
(3) Glacial	10^4 years	The phases within an ice-age, e.g., the duration of the Würm was of the order of $2{\cdot}5 \times 10^4$ years
(4) Minor geological	10^6 years	Duration of ice-ages as a whole; periods of evolution of species
(5) Major geological	10^8 years	e.g., ice-ages at intervals of $2{\cdot}5 \times 10^8$ years

Principal bases of evidence

(1) Instrumental; behaviour of glaciers; records of river-flow and lake levels; non-instrumental diaries; crop yields; tree-rings (also for dating).

(2) Earlier records of extremes; fossil tree-rings: archaeological finds; lake levels; varves and lake sediments; oceanic core-samples; pollen-analysis; radiocarbon dating.

(3) Fauna and flora characteristic of interglacial deposits; pollen-analysis; variation in height of snowline and extent of frozen ground; oceanic core-samples (dating through latter).

(4, 5) Geological evidence; character of deposits, fossil fauna and flora; dating largely through radio-activity of rocks.

Now we can plot the flux in time of such things as monthly means of temperature, just as we can plot the average flux of the daily means throughout the year and then argue about the tendency for peaks and hollows to occur with considerable consistency at certain dates. If we plot monthly means over long periods, preferably with the aid of a little smoothing, e.g., to decadal running means, the resemblance of the flux in time to the visible flow of a turbulent river in space is noticeable (cf., Fig. 1). It is evident that we want to learn more about climatic flux, which clearly starts as the product of the small but definite irregularities which are observed in the earth's general atmospheric circulation from year to year. Some of these irregularities, like those of the glacier-fed torrent, appear to the observer to be virtually at random, but the persistence of any results therefrom cannot be prolonged owing to the energy of the following water which rapidly damps out any brief train of waves developed in the stream. This aspect of the periodicity-hunts so fashionable early this century becomes apparent from a study of the well-known paper by Sir David Brunt (1937).

But if a sufficiently large obstacle is dropped in this metaphorical stream or flux

of events in time, such as for example an ice-age, for how long do its effects last, and how much of the stream is affected ?

It will be evident from pursuit of such a space-time analogy that what we need, as spectators endeavouring to learn about the behaviour of the earth's atmosphere over long periods of time, are more precise determinations of the amplitude of variations of the climatic elements at various periods in the past, and over a wide range of latitude. For a limited part (albeit the most interesting part) of the earth's crust we are beginning to be able to study in some detail fluctuations of the first order in Table 1 above. We are gradually approaching the mapping of the fluctuations in the periods of the second order, but only by what may be called ' stream-gauging ' methods; i.e., we know the general variation in the volume of the stream but not where the minor crests and hollows occurred, nor what was their magnitude. We lack the capacity to compile running means.

For the periods of magnitude (3) what we want to know is whether the variations whose amplitude we know of under (1) and (2) were maintained, or whether they were amplified or damped out. Argument from analogy with the present day is difficult. If in certain parts of the world the minor climatic variations over periods of a few years were the same in the maximum of the Ice Age as they are today, we might then argue that additional energy must have gone into the system to overcome the braking effects, and a basis might thus be found for postulating variations in the radiation reaching the earth's surface due to some extra-terrestrial cause. This might prove welcome to Sir George Simpson and to Drs. Hoyle and Lyttleton. But we are a very long way indeed from such perfections. As for the variations of climate of higher order (4) and (5), while the geologists are still in dispute regarding the relative extent of land and sea, we can do little more than record the evidence which they provide of temperature, of seasonal range, and occasionally of wind speed and rainfall. For earlier geological periods the evidence has been so well summarized by Dr. C. E. P. Brooks (1949) that it is unnecessary to go further in this essay.

We should instead remind ourselves that the subject of climatic variation must largely be studied in relation to events over the last 25,000 years (orders (1) and (2)) and that such studies are likely to bear especially welcome fruit as they affect the welfare of *Homo sapiens* whose evolution owes so much to the environmental stresses of that period. Secondly, models for the events of that period can largely be found at the present day, as in combination with other evidence, the present existence of large ice-caps leads us to believe that we are living in the later stages of an interglacial phase. Thirdly, in order to study what is essentially turbulent flow, we want greater precision of measurement of the time of occurrence as well as the amplitude of meteorological events. That we are making progress is evident if we consider the advances made during the past 200 years; instrumental climatology began when there was a sufficiency of meteorological records to discuss and this was not until far on in the 18th century.

The growth of knowledge of climatic variations

Even before the eighteenth century, students of the classics were aware that there could be no fundamental difference between the climate of Ancient Greece and Rome and that reported by contemporary travellers; this was evident from a comparison of agricultural practices. On the other hand, it remains a perennial temptation to mankind to attribute the blame for many shortcomings to causes outside man's control; and in regard to climate successive generations retain to this day an inclination to believe that their fathers benefited by easier conditions. Elsewhere the romantic tendency for hero-worship has led others to presume that their ancestors endured and overcame fiercer extremes than their modern descendants.

Such divided opinions would not have prevailed if any really serious change had occurred; it would have been extensively recorded by historians. As it is, historical writers have repeatedly been prone to discuss environmental factors from their own too brief experience. We are fully aware how, at the present day, the occurrence of a few years' drought in a particular area can set in motion vast trains of discussion; the drought of 1933-36 in the Middle Western States may be instanced. We have heard much less of that area, or its possibilities, during the past decades of more reasonable rainfall. The dimensions of our minor fluctuations, those of the first order of magnitude, are evidently quite sufficient to influence popular writing, and we may presume that in the absence of instrumental records, classical and mediaeval historians in marginal areas would produce very varied opinions of climatic variation, not free from subjective bias.

Instrumental observations on a reasonably widespread scale were beginning to accumulate by 1770 among the countries of Europe and their colonies. Tolerably extensive tables of monthly means of temperature and rainfall were assembled by Père Cotte (*Traité de Météorologie* 1774), and the distribution of climates was discussed by Kirwan (*Essay on Climates*, London, 1787). Precision with regard to distribution of the climatic elements was however very slowly attained; the gradual evolution of our familiar isothermal maps from Humboldt, in 1817, through the German and Scottish physical geographers, the gradual association of climatic environment with those vegetation and human types most fitted to survive, was a development of the Darwinian epoch. Huntington's endeavour to relate the performance of man to climatic environment represents its last flowering. Through de Candolle and Schimper we were led to Köppen's repeated re-shapings and polishings of his climatic regions, finally summarized as late as 1931 (*Gründriss der Klimakunde*). Echoes of that period are still to be heard from America, where Thornthwaite and his colleagues are producing further interpretations, and subdivisions of the world's climates. Geographers whose business it is to draw maps are still here-and-there engaged in lively controversy with regard to the validity and usefulness of these varied interpretations, that is, they are discussing the location of climatic variations in space.

The principles of distribution of the outstanding types of vegetation in relation to climate are tolerably well known. As these types can be well identified in the geological record, we have a key to the general character of the climate prevailing over great stretches of geological time. For example the Eocene flora of Grinnell Land testifies to a more temperate climate; for this and other periods, the evidence is sufficient to have led long ago to C. E. P. Brooks's (1925) convincing demonstration of the nature of the polar climate throughout the greater part of geological time.

The growth of knowledge of the evolution of climate on the geological time-scale has paralleled the growth of geology which has so largely taken place since 1800. The identification in the 1830's of the evidence for Pleistocene glaciation in Britain; the first rough datings later in the century; the magnificent step forward represented by the Swedish geochronology (1910) have provided us with a picture of very emphatic climatic variations over the past 25,000 years, more particularly in northern Europe; this has been supplemented by parallel investigation in North America. Yet during the past century classical studies have been supplemented by the archaeologist in such a way as to indicate that the magnitude of the overall variation in the past 5,000 years must have been quite small. Much of the stimulus in this field derives from Arago, who in 1834 propounded the view that the climate of the Mediterranean world had not significantly changed in historic times (cf., chap. VI, Napier Shaw, *Manual of Meteorology*, I, 1926). Archaeological sources have provided evidence such as that of the altitudinal range of movement of animals and man which over long periods of time does not point to any serious difference from present-day conditions. Yet in marginal areas classics and romantics

can still select material in support of their opinions regarding the effects of small climatic changes; a large number of earlier contributions were summarized by Gregory (1914) with abundant references.

By 1921, then, apart from the earlier geological record, our knowledge of climatic variation in NW. Europe pointed to :

(*a*) a major positive change in the past 25,000 years; the dimensions of which could be approximately judged by comparing the present-day altitude of the snowline with that associated with the known extent of past glaciation;

(*b*) minor prehistoric fluctuations, principally based on the analysis of the types of vegetation found in bogs, indicating that an ' optimum ' had been attained between 5-7,000 years ago and that an appreciable deterioration occurred at some time between 500-850 B.C.;

(*c*) a variety of interpretations of the historical evidence, leading many to consider that while over much of Europe no serious change could be recognized, the changes were sufficiently marked on the desert and polar margins to affect the conditions of human occupation;

(*d*) a variety of interpretations of the instrumental evidence, with a multiplicity of ostensibly regular periodicities claimed by many investigators.

The state of knowledge was well summarized by C. E. P. Brooks (*Evolution of Climate*, 1922); the results of periodicity-hunts were unforgettably listed by Napier Shaw (*Manual of Meteorology*, II, 1928, p. 320). From this stage we may review the modern developments, remembering that the past existence of a 'Climatic Optimum' was by then already established for NW. Europe, with a summer mean temperature of the order of 3°F to 5°F above that prevailing today. In what follows the main emphasis will be laid on the variations of temperature, as it seems probable that in cooler temperate latitudes this represents the master factor in regard to vegetation and other climatic indicators such as glaciers.

Climatic variation: developments since 1921 *with regard to fluctuations of the second order*

The new technique now available for the student of past climates both before and after the Optimum was provided by pollen-analysis; a succinct account has been given by Godwin (1950). Since the last phase of the Ice Age, of which the dated retreat-stages have been summarized by Wright (1937) and Flint (1947), we have obtained a picture of the forward march of tree-vegetation towards Scandinavia and Laurentia, with intervals during which progress was checked or there was actual recession. Pollen-analytical studies of the stratified deposits in lakes and bogs have been carried on throughout central and NW. Europe (cf., Firbas 1949, Godwin 1947, also Movius 1942). Inasmuch as the cold tree-line is tolerably closely associated with certain temperature limits (Köppen 1931, Zeuner 1945, Poser 1947, Miller 1950, Büdel 1949) for the summer months, numerous investigations have long ago seized on the fact, that the tree-line in the Ice Age lay across central France, as a starting-point indicating a July mean of 50°F (10°C). From this, the subsequent position of the same July isotherm might be estimated for successive phases in the Late and Post-Glacial before the climate had attained its optimum warmth. Such an attempt has been made by the present writer (Manley 1949, 1951). Yet such values must perforce be very approximate; we do not know how much lag must be allowed between the meteorological events and the results indicated by the profusion of different types of pollen at various horizons; and, in cooler climates, local climatic differences arising from relief and the characteristics of the soil become more effective. The complexity of the factors involved in regard to advance of the northern limit of tree-growth is illustrated by Hustich (1949). Further, the development of an open forest cover

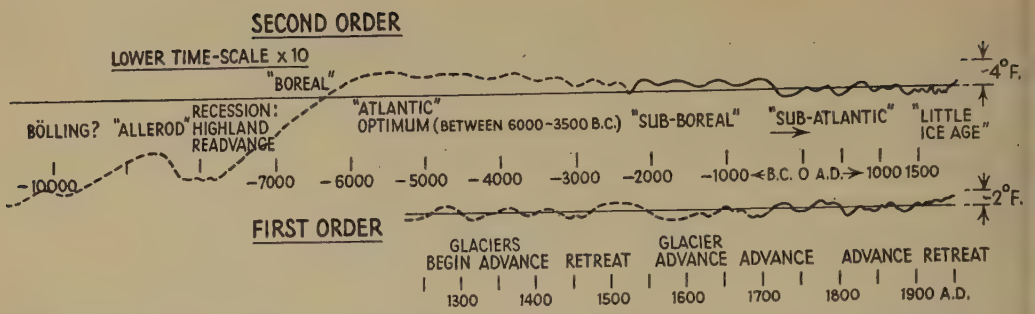

Figure 1. Sketch of the fluctuations of the average temperature of the summer months in England. The above indications cannot yet be regarded as exact either for temperature or dating. Based on a general consideration of data from Brooks, Conway, Flohn and others.

would itself lead, in a breezy climate, to further amelioration of the summer temperatures near ground level. In NW. Europe this might well amount to 1°F on the monthly means, if the comparison with urban climates today is acceptable. Meteorological theorists must therefore use these values with caution.

The distribution of tree vegetation is moreover not by any means a complete index of prevailing climate. Botanists know the pitfalls and, in a valuable paper before this Society, Faegri (1950) has made them very clear. As he says, the difficulty arises because the meteorologists who can judge the conclusions are not, in general, competent to judge the value of the evidence.

Yet for certain stages in the Post-Glacial more detailed analysis of the prevalence of pollen of species of plants with particular requirements, such as holly, mistletoe, and ivy in lowland Denmark (Iversen 1944) is capable of setting close limits to the values of both summer and winter temperature. The trouble we then face is that of more precise dating of the periods during which these species flourished. Nevertheless, it does appear that we have a valuable method capable of extension to other western European countries; Iversen too pays attention to the need to eliminate local climatic effects in regard to the incidence of frost. More recently this necessity has been discussed by Dahl (1951) who notes that in mountainous country close correlation between the macroclimatic factors and the distribution of plants is not to be expected. Iversen's estimate of the fall of the summer temperature since the Climatic Optimum appears to receive support from S. Norway (Hafsten 1950).

Further, we have scattered evidence from shell-fish, land mollusca and the like which already goes to provide rough values for the sea temperature at periods broadly contemporary with those to which such findings as those of Iversen for Denmark apply. That being the case, as we know that from the Climatic Optimum onward the extent of land and sea was much the same as today, we can set a pattern to which the Post-Glacial variations of the summer temperature in an off-lying island such as Britain are likely to conform; again, we are making the general assumption from the size and growth of trees that over the last 7,000 years the character of the atmospheric circulation over NW. Europe has not seriously varied in respect of wind speed, frequency of rainfall and the like, so that we might expect the gradients of mean temperature to resemble those of the present day. Estimates of the winter mean temperature in Britain cannot be so easily approximated from values given for Denmark, as the east-west temperature gradient at that season is much more affected by slight variations in the circulation. Broad indications however can be derived, e.g., for southern England for periods in the later

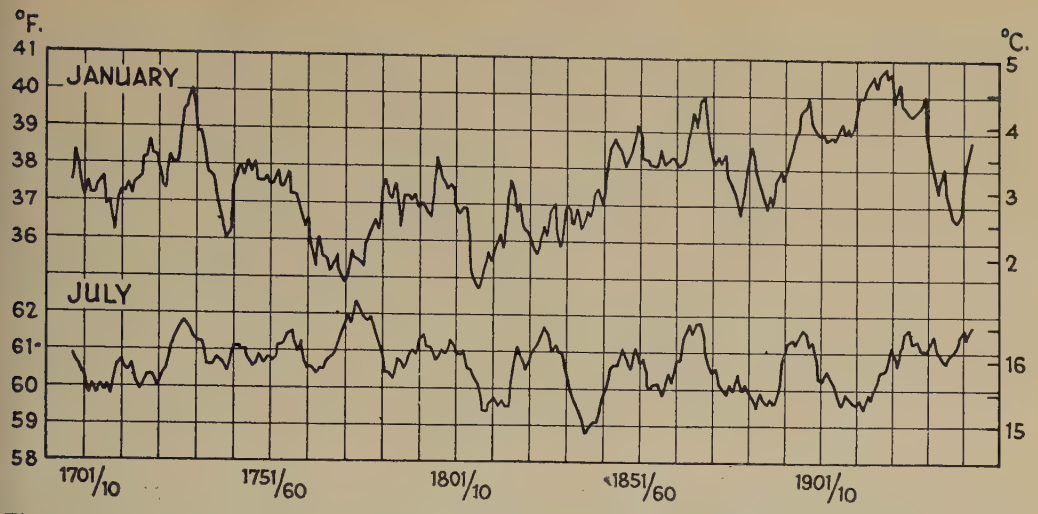

Figure 2. Ten-year running means of temperature, 1698-1952; representative of central England, January and July (based on Manley 1953).

Neolithic and also the Iron Age, from the growth-rings observed in fossil charcoal. These testify to temperatures, to summer rainfall, and to an annual range differing little if at all from the experience of the present day; but the evidence so provided must be viewed with caution (Tansley and Godwin 1941) and long intervals of time remain in which considerable fluctuations might have occurred.

We can nevertheless set, as the result of recent work, quite close limits for the summer mean temperature prevailing at and after the Climatic Optimum; and with time these data are likely to be supplemented for more places, giving us a greater opportunity of setting similar limits for winter means.

The two diagrams (Figs. 1 and 2) are intended to illustrate the probable nature of the fluctuations of mean temperature in Britain, so far as our evidence at present goes. In the first of these diagrams, first- and second-order fluctuations are shown together, the time-scale for the one being ten times that of the other.

The next stage is to comment on the evidence for the curves in Fig. 1, by way of showing that as yet they are to some extent pictorial, and lack the precision which many meteorological theorists would wish to have before committing themselves to any explanation.

Estimates of temperature and of seasonal range before the Climatic Optimum

Estimates of the mean temperature during the Ice Age in various parts of Europe have been made long ago, for example by Clement Reid, who made use of the depression of the snowline, recognized to be of the order of 4,000 ft in the Alps, and of the evidence for permanently frozen ground near the periphery of the ice sheet. The necessary mean annual temperature for the latter phenomenon can be judged in Siberia, Alaska and Canada; it is however not known very exactly, and values have been given ranging from 26°F to 31°F (Muller 1947); compare also for Canada, Jenness (1949).

A more risky attack was made on the range of mean temperature from summer to winter, in the famous early papers by C. E. P. Brooks, summarized in his *Evolution of Climate* (1922). Therein he applied his conceptions of 'continentality' to former distributions of land, sea and ice, in order to argue that in SE. England for example at the height of glaciation the July mean temperature would be about 13°F below that of today and that of January about 18°F below. The fundamental assumption here made,

we must note, is that in the region of the British Isles the general vigour of the atmospheric circulation, i.e., the storminess and windiness would be not less than that prevailing today; and would correspond with observed conditions near the margins of temperate ice-caps adjacent to open seas in disturbed latitudes. For it is fundamentally recognized that at the height of glaciation the Atlantic a few miles off our British shores was open water, in which drifting ice was melting (cf., Flint 1947).

While there are general reasons for supposing that this assumption is justified, we are as yet unable to confirm it. That the development of widespread ice-sheets would result in the atmospheric circulation in adjacent areas between them and the great oceans being in all probability more vigorous than today is a point already made; and this view has received quite recent support from Willett (1950b). It is also in agreement with the general theories put forward by Simpson (1934). Nevertheless, the problem is to explain how far inland the effects of that coastal vigour might be expected to penetrate; for on this will depend the winter mean temperature and the likelihood of precipitation.

Some support for Brooks's estimates of the seasonal range at the height of the Würm, or last major phase of glaciation, in the periglacial region of SE. England, is however forthcoming from other sources. The arguments for summer temperature in relation to the distance from the cold tree-line in France have already been mentioned. We are, moreover, aware that in Britain permanently frozen ground does not appear to have existed much further west than the West Midlands, which gives a rough value for the mean annual temperature. From this, it would appear that the mean temperature of January at the height of the Würm glaciation could be set as high as 15°F for the London area (Manley 1951) but it could be put lower (Klute 1951, suggests mean 5°F), and in any event the winter mean temperatures must have been capable of very wide variation. This might go far to explain the remarkable development of what some are disposed to call ' cryoturbation ' (frost-heaving and allied phenomena); at the same time, Estienne (1952) has made it clear that one need not always invoke *permanently* frozen ground to satisfy the evidence. Nevertheless it is clear that one of our most urgent needs before we can carry forward discussions of the energy of the atmospheric circulation in the Ice Age is a better estimate of the gradient of the mean winter temperature, inland from the Atlantic coasts. Similar estimates of the gradient of the winter mean temperature between the shores of the Gulf of Mexico and the edge of the Laurentian ice-sheet would likewise be very helpful.

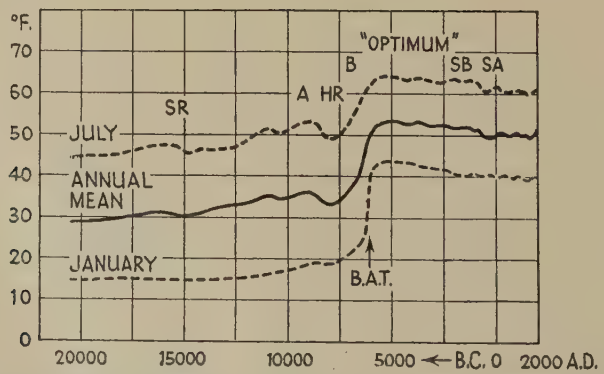

Figure 3. Estimated trend of mean temperatures in England. SR = Scottish Readvance (Götiglacial ?), A = Allerød phase, HR = Highland Readvance (' Finiglacial '), B = Boreal, BAT = Boreal-Atlantic Transgression (rise of winter means), SB = Sub-Boreal, SA = Sub-Atlantic. Dating is still very approximate. The above represents part of a third-order fluctuation, complicated by the effects of a marked extension of the area of the sea. Compare July at Erfurt (Klute 1951, p. 280).

In Fig. 3 a number of estimates of the mean temperature of January and July for different periods have been put down. The evidence for these, both as regards time and amplitude, should be discussed in order to illustrate the imperfections which must perforce be attached to our present estimates.

In regard to time; the Swedish geochronology indicates that the Scandinavian and British ice began to retreat at some date of the order of 20,000 B.C. from the region of the Baltic coast and, in Britain, from the line marking the generally accepted limit for the Newer Drift. About 14,800 B.C. the retreat was halted, and in places there was a slight readvance ('Scottish Readvance' in Britain, *Götiglacial* in southern Sweden). While caution is still necessary, it may be mentioned that in recent years these correlations have become widely accepted. After some centuries the retreat was resumed and continued, apparently with minor oscillations and halt-stages; there is growing evidence of a minor 'Bølling' oscillation (cf., Faegri 1935), which we may perhaps find to be linked with the Lammermuir-Stranraer moraines in Scotland. About 8,500 B.C., however, a further climatic recession of some magnitude occurred. In Britain the glaciers, now for the most part confined to the Highlands with perhaps small outliers in the mountains to the south, readvanced; the cold tree-line, marked by the presence of birches in the Scottish Lowlands, retreated southward to the region of the southern English Midlands, and likewise from southern Norway to Holstein. This relatively warm 'Allerød' phase and the 'post-Allerød' climatic recession is so well marked over northern Europe as to form an extremely important datum for the discussion of the reasons for climatic variation; moreover, radio-carbon dating has confirmed the development of a similar climatic recession across the Atlantic. But our evidence does not yet allow a precise estimate of the amplitude of the oscillation of temperature thus implied. Radio-carbon dating is not sufficiently definite to establish whether there was a significant lag between the two sides of the Atlantic, although it does appear from the recent dating of the Windermere sediments (Pennington 1947) that, in Europe, its results developed with regard to vegetation at approximately the same time in Scandinavia and Britain; this cooler phase, moreover, lasted about five centuries. Subsequently the climate became warmer with apparently little serious interruption until the end of the Boreal; then, with the marked broadening of the European coastal seas about 6,000 B.C. and further invasion of Atlantic water, the winters became much milder and the Climatic Optimum was gradually attained. A convenient summary correlation of chronological events in northern Europe is published in the *Bulletin of the Geological Society of America* (1949), **60**, No. 9, devoted to Pleistocene research, and another by Lacaille (1948).

Agreement however is not yet reached regarding the correlation of events in central Europe. Firbas (1949) reports a warm interval of similar character to the Allerød phase from the north side of the Alps. Elsewhere, however, the correlation and dating of geological and botanical events in the Alps does not appear to be securely established; for a discussion of this subject with further references see Conway (1948), also Zeuner (1949, p. 90). More recently, core-sampling of the sediments on the floor of L. Zürich has been carried out, of which the results are eagerly awaited.

So much for the timing of events; now for the seasonal range and the amplitude, with special reference to Britain, of the temperature fluctuations of the second order before the Climatic Optimum.

Estimates of mean temperature based on the extent of glaciation

Primary estimates of the fall of mean temperature in the Ice Age were made, as has already been said, from the known effects on the snowline which were taken to imply an overall fall of mean temperature at sea level of the order of 12°F in the Alps, and

correspondingly more towards the centre of the great Scandinavian ice-sheet; a reasonable deduction in the light of our knowledge of the radiative properties of existing ice-caps. But increased glaciation can take place not only as a result of fall of temperature; it may follow an increase of precipitation, provided that the latter falls as snow and that the winter accumulation is in excess of summer melting. Ahlmann's prolonged studies have however demonstrated that, round the North Atlantic, temperature variations are the more important factor governing the advances and retreats of glaciers. More particularly, the temperature prevailing during the ablation season is significant as, in combination with the total snowfall, it affects the altitude likely to be reached by the firn-line, or climatological snowline. For central and northern Norway the relationship has been demonstrated in the diagram (Fig. 4) below, reproduced from Ahlmann's (1948) general account of his work.

The further demonstration, that the greatest development of glaciation in Scandinavia, Iceland and the Alps in post-glacial times has culminated during the past 250 years, agrees with the evidence from pollen-analysis that the effect in regard to temperature of the recessions of the past 5,000 years has exceeded that of the recoveries. Further, it presents a remarkable opportunity to integrate the climatic fluctuations of first and second order, as shown by Callendar (1950).

For it follows that, in cloudy and windy maritime climates at least, estimates of the mean summer temperature likely to prevail at the snow-line can be made if we know the water equivalent of the annual precipitation. Alternatively, if we know the mean summer temperature, we can estimate the annual precipitation. Thirdly, other evidence may be assembled which contributes to a knowledge of the probable height of the snow-line at certain periods.

It may incidentally be added that Finsterwalder (1952) has devised photogrammetric methods of survey of glaciers from which the variations in the position of the glacier

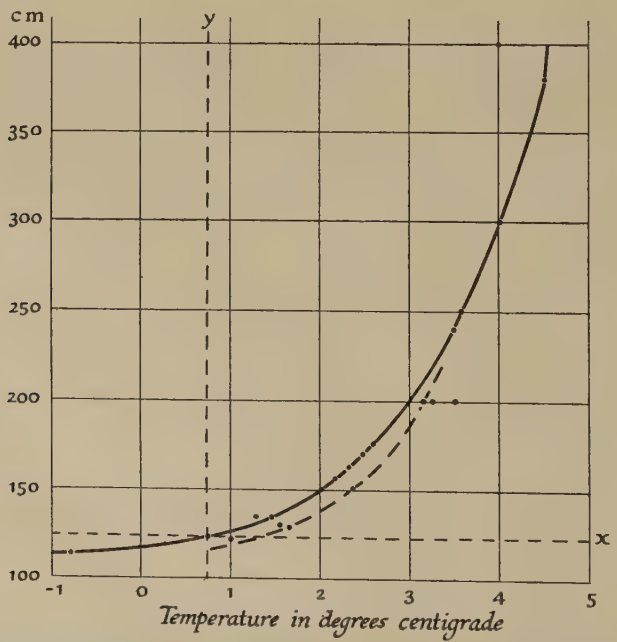

Figure 4. Ahlmann's relationship between the summer mean temperature and the water equivalent of the snow-accumulation, at the glaciation limit in certain districts of central and northern Norway (the glaciation limit is in general nearly the equivalent of the climatological snowline). Reproduced by courtesy of the author and of the Royal Geographical Society from Ahlmann (1948).

snout can be used over a period of years to provide an index of the changes of average summer temperature during advances or retreats. This should prove useful in more remote mountain ranges where long-term meteorological records are lacking, provided that suitable photographs can be found and moraines dated.

To illustrate the possibilities inherent in the assemblage of data with regard to the snowline, the present writer's discussion (1949) of the post-Allerød climatic recession will serve. From the Windermere sediments (Pennington 1947) and other sources, in the previous Allerød phase, the birch is known to have grown at least as far north as the Scottish Lowlands, whereas during the colder phase it retreated to the SE. Midlands. If we can accept a gradient of summer mean temperatures from SE. to NW. similar to that which is shown by our isothermal maps today as we move through Britain or Norway towards the north-eastern Atlantic, it is reasonable to suggest a July mean during this recession of about 47°F for the Windermere region, roughly 200 ft above sea-level. With such a July mean and an atmospheric circulation broadly similar to that we now experience, the June-September mean in that region would be approximately 44·5°F.

From Ahlmann's curve (Fig. 4), it would however appear that in similar cloudy and windy mountain regions, the mean temperature for the four months June-September should be 36·5°F at the snow-line, provided that the water equivalent of the annual accumulation of snow at that level is 175 cm or 70 in, i.e., the annual precipitation should lie between 80 and 85 in. In the light of what we now measure over much of the Lake District, this is a reasonable assumption. Given the prevailing lapse-rate of about 1° for 280 ft, the snowline on the glaciers should therefore have occurred at an altitude corresponding to 2,500 ft at the present day. But it is also to be observed that more locally in the central part of the Lake District the precipitation would be larger; and that a further 30 per cent increase would allow the temperature of June-September to be nearer 38°F. This would in turn allow the firn-line to be as low as 2,000 ft, which would allow considerable glacier development in the corries, more particularly those with a higher base-level. Some estimates of the precipitation are given by Klute (1951).

Now we do know from the varves in the Windermere sediments that small glaciers must have existed during this post-Allerød climatic recession; that the last moraines representing their limits of advance can be seen at the head of several of the central valleys in the regions of highest precipitation, and that their position and size in relation to the available gathering-grounds accord excellently with the assumption of a snowline at 2,000 to 2,500 ft. Recent studies of the sediments of the former Kentmere lake (Walker, D., not yet published) and of the neighbouring moraines do not appear to be at variance with the above assumptions.

Arguments such as the above can be presented for the several earlier stages of halt or minor readvance within the general retreat (Fig. 5) and need not be repeated here (Manley 1951). The arguments for winter temperature depend on the assumption that with increase of summer warmth the sea must have been increasingly effective in winter as well. It will however be evident that we are dealing with a series of approximations, and we are in any event assuming that the climate was at least as disturbed as at the present day. The inter-relationship of a number of variables must be so contrived as to fit the field evidence, which is in turn derived from scattered sources and a number of sciences, and is not yet invariably accepted. For example, what some have claimed to be moraines may by others be interpreted as minor landslips below glaciation level. It will therefore be observed that the validity of these approximations is such that they must emphatically not be overstrained by the meteorological theorist.

In the present state of our knowledge it certainly appears that each of the climatic recessions, represented by the halts or minor readvances within the general retreat of the

Figure 5. To illustrate the limits of glaciation for the several phases of the glacial retreat (A-D) in the British Isles (reproduced by courtesy of the Royal Geographical Society).

TABLE 2

(a) Estimated July and January means (°F) at sea-level, on a climatological basis (Source: Manley 1951)

Stages :	A	B	C	Allerød	D	Present day
Outer Hebrides	40-23	40-23	42-25	46-25	44-25	56-42
Ben Nevis	40-15	40-15	42-17	48-17	45-17	57-39
Lake District (inland)	42-12	43-12	45-16	52-17	47-17	60-39
Kent-Sussex (inland)	45-15	47-15	49-17	54-19	54-19	62-39
South Cornwall	46-24	46-24	48-28	53-31 ±	49-31 ±	61-45

(b) Estimated average altitude of the climatological snowline or firn-line (in feet)

Ben Nevis	±	400	500	1,100	2,900	1,900	(5,300)
Lake District	±	1,000	1,100	1,700	3,500	2,500	(5,900)
Snowdonia	±	1,400	1,500	2,100	—	2,900	(6,300)
Brecon Beacons	±	1,750	1,850	2,450	—	—	(6,650)
Dartmoor	±	2,100	2,200	—	—	—	(7,000)

last glaciation, implies a change in the average temperature of the summer months of the same order of size as that we can observe today between groups of more and less settled summers, which may well be associated with oscillation of the zonal index in one direction or the other. Moreover, as we shall see, they are of broadly the same climatic dimensions as the several recessions since the Climatic Optimum which have been sufficient to have widespread effects on the tree-vegetation in Scandinavia. But we cannot yet say that these recessions were not ushered in by short-term fluctuations of considerably greater magnitude, whatever their long-term appearance. In other words, we need to know more about the possible effects of first-order embroideries superimposed upon the second-order oscillations whose amplitude we are beginning to be able to estimate.

Estimates of temperature and seasonal range from the Climatic Optimum onward

Careful examination of many pollen-diagrams has shown that, in Scandinavia at least, the climate has deteriorated in a manner which has recognizably affected the vegetation on six or more occasions since the Optimum (von Post 1946). In each case the pollen-diagrams indicate changes of a type which would ensue following a change in the climate in the direction of cooler and more cloudy summers, which, in turn would be likely to be associated with increased available moisture, whether through increased rainfall, diminished evaporation, or both. In an avowedly speculative paper which is of the greatest value to the thoughtful climatologist V. Conway (1948) has pointed out that several of these 'thresholds' have been crossed at intervals of the order of 900 years; the dates given approximate to 3200, 2300, 1400, 500 B.C., 400 and 1300 A.D. Yet on the other hand the July temperatures given by Iversen do not indicate a fall in the prevailing mean temperature, from the Sub-Boreal to the Sub-Atlantic for example (18°C to 16°C in central Denmark; present-day 16°C) greater than one would expect to find between a characteristically unsettled, cloudy and rainy summer and a fine one at the present day. Moreover, this fall (of about $3\frac{1}{2}$°F) across the most marked of the 'thresholds' is not more than about twice that which prevails between decadal mean values within the instrumental record.

Inasmuch as such differences tend to be less marked in more maritime climates it is no matter for wonder that some of these vegetational recessions are not so readily recognized in Britain as in Scandinavia. Further, in the light of other evidence, assembled by C. E. P. Brooks (1949, p. 305), we must be ready to assume that there were in addition other irregularities of shorter length. In drawing the upper curve in Fig. 1, I have added to the assumed effects around the 'threshold' dates above, minor fluctuations of mean summer temperature based on the indications of prehistoric periods of wetness and dryness given by Brooks. The resultant picture is not likely to be accurate, but will serve to show that the characteristics of the second-order fluctuations on their time-scale appear very similar to those of the first-order fluctuations on their scale. Moreover we must recognize Faegri's dictum: a short-period oscillation of temperature might not affect vegetation, but a longer-lasting oscillation of equally small magnitude might show decided effects (Faegri 1950).

Some tenuous evidence with regard to the variability of mean temperature in England across the last two of these thresholds can be assembled from the historic and archaeological records. C. E. P. Brooks has shown that both before and after 1300 A.D. there have been fluctuations of rainfall of the type operative over spells of 20-25 years, such as we can recognize within the instrumental period of the last two centuries, and therefore one might surmise not greater in magnitude. No very emphatic changes can be recognized with regard to the frequency of ice in the Baltic, or other similar index of winter temperature. Therefore it would appear that in England at least the change in mean temperature between the 13th and 14th century was quite probably less than 2°F, of the same order as that which we can recognize between blocks of 30 years of annual means during the past two centuries (cf., Lysgaard 1949) which have not been sufficient to draw widespread mention in our histories. That the contemporary change in mean temperature in western Scandinavia might be greater is however suggested by what we know of the comparisons within the instrumental period.

Again, although the temperature in England can have changed but little, there is appreciable evidence that the climate became effectively wetter towards the end of the Roman occupation. Recent work in Somerset (Godwin 1948) appears to support this; further support can be found in a recent discussion of the southward spread of corn-drying by means of kilns (Scott 1951).

The broad results of pollen-analytical studies in NW. Europe certainly indicate that the changes since the Climatic Optimum have been slight; in other words, that the fluctuations of second order do not appear likely to exceed in magnitude those of the first order. From the evidence of glaciation, however, it appears that a slow overall cooling has occurred which is part of a fluctuation of the third order (10^4 years) for which, at present, it appears that we may have to seek an answer in slow variations within the ocean – which may even be associated with isostatic processes.

Radiocarbon dating

This depends on the established fact that the proportion of carbon-14 to carbon-12 in dead organic matter is a direct function of the time elapsed since the death of the organism. The apparatus used has to be extremely sensitive; determinations are very slow, and the greatest care must be taken to ensure that the sample under analysis is free from contamination resulting from the introduction of carbon from other sources. Zeuner (1950, 1951) has given an account of the method, and the difficulties. The method is limited to material less than 20,000 to 30,000 years old, and any date so determined is subject to errors of the order of $\pm$ 200-700 years. About 120 dates have so far been determined from the Americas, about 40 refer to material from the Old World (McBurney 1952). Some of the dates so obtained can be checked against the Swedish varve-chronology or other sources. The accordance in general is good enough to enable us to look forward to the ascertainment of the approximate age of other Late- and Post-Glacial deposits; but the chances of error are such that it does not help us very far as yet with regard to the climatic variations which we have described as second-order (250 to 900 years). For the student of climatic variation, however, the most important result is that discussed by Flint and Deevey (1951), namely, the close correspondence in time between one of the latest halt stages of the retreating Laurentian ice and the post-Allerød climatic recession in Europe already discussed. The dating of this re-advance near L. Michigan to about 9,000 years B.C. $\pm$ 350 years is considered to correspond sufficiently well with the accepted dating of the recession in Europe (considered in Sweden to have set in about 8350 B.C.).

Clearly we are a long way from such precision of dating that we are able to say at what rate the mean temperature changed, and whether it did so over the same time-interval in different parts of Europe, let alone across the Atlantic. Yet if we find that there was an appreciable lag between events so distributed, it becomes less easy to accept the views put forward by Willett with regard to variations of the zonal index, attractive though they unquestionably are. If such high- or low-index circulations persisted over longer periods of time and were superimposed upon a slow rise or fall of the sea surface temperature over the Northern Atlantic, they would lead to temperature changes of the dimensions required by the earlier retreat stages of the glaciation, the Bølling and Allerød oscillations and the post-Optimum climatic recessions. Whether they did, and if so why they occur, must await further field evidence; the greatest promise would appear to lie in the deep-ocean core-samples now under investigation (Ovey 1949; Wiseman and Ovey 1950).

An illustration of the value of the present as the key to the past has been provided by the events of 1947. The immediate result can be seen in the stimulus given to the development of the theory of the action of ' blocking highs ' such as were only too prominent in February and August of that year in NW. Europe (cf., Rex 1950, Brezowsky and others 1951). Further development is well represented by Willett's recent paper (1950b) on ' The general circulation of the last (Würm) glacial maximum ' and by his contributions already cited, to the *Centenary Proceedings* of this Society (1950) and the *Journal of Meteorology* (1948).

But while such theories can be formulated, it is fair also to note Willett's own words (1950b) :

' this maximum glaciation pattern throws no light on the problem of the initiation or the early stages in the development of an ice age . . . . this question is left unanswered as to the degree to which the major climatic changes of an ice age are a preceding or a contemporary cause of glaciation on the one hand, or a contemporary or subsequent result on the other hand.'

In other words, we do not know enough about rates of change and lag effects, and in the present state of knowledge regarding dating, the second-order climatic variations are not likely to help us. As for the variations of higher order, we are still mapping. Hence we need as yet to preserve a cautious outlook even with regard to theories of late and post-glacial change. While their magnitude may agree with that which is found useful in connexion with current theories of the circulation we need to find out much more about the incidence and relative magnitude of the several climatic recessions on either side of the Atlantic and their repercussions in other parts of the world, especially the equatorial tropics. With regard to our second-order fluctuations we are thus brought back to the question of dating, at least to something nearer than 100 years; and of determination of mean temperature to finer limits than the 2°F or 3°F within which so many critical variations in NW. Europe appear to take place. It is by no means certain whether the newly-developed oxygen-isotope method (Urey 1948), mentioned again in a later paragraph, will get us any closer; the carbon-dioxide method of Wiseman applied to ocean-cores may prove more helpful.

While there are these uncertainties we are far from being able to discuss the relationship between first-order and second-order fluctuations. Let us proceed to review our knowledge of the small, first-order fluctuations whose time-scale is of the order of 10 years and whose distinctly irregular length appears nevertheless to centre tolerably frequently around an average of 20 to 25 years. This length can of course be associated with the solar activity period, and has been noted by others (Brezowsky, Flohn and Hess 1951; also see Schulman 1951).

Climatic fluctuations of the first order

Recent years have seen a marked revival of interest in the available long-period instrumental records of temperature, rainfall and other meteorological elements. The most remarkable of these was compiled from a number of records in Holland during the occupation (Labrijn 1945); it provides with other things a table of monthly means of air temperature representative of Utrecht from 1706 down to the present day. An English series has now been carried back to 1722 and, with the aid of Utrecht, to 1698 (Manley 1953). Other compilations have been made and used by Ahlmann (1949) in his discussion of the evidence for the present climatic fluctuation. A particularly extensive assembly and discussion of the long-term records from all parts of the world has been made by Lysgaard (1949) and has received further discussion by Willett (1950a) and Wallén (1950). Reference should also be made to the later discussion published by Labrijn (1950) in which he illustrates the remarkable correspondence between the decadal running means of the range of temperature January-July for a number of widely separated European stations at which records go back to 1750. The extent of this correspondence north of the Alps goes far to reassure those who distrust the value of reductions of thermometer observations before 1800; Labrijn's illustrations of the trend for four stations is reproduced below (Fig. 6).

On the other hand Lysgaard's curves of 30-year running means for January and July for similar European stations are not markedly parallel; some will ask why they should be, others will be led to suspect the standardization of the earlier reductions. It

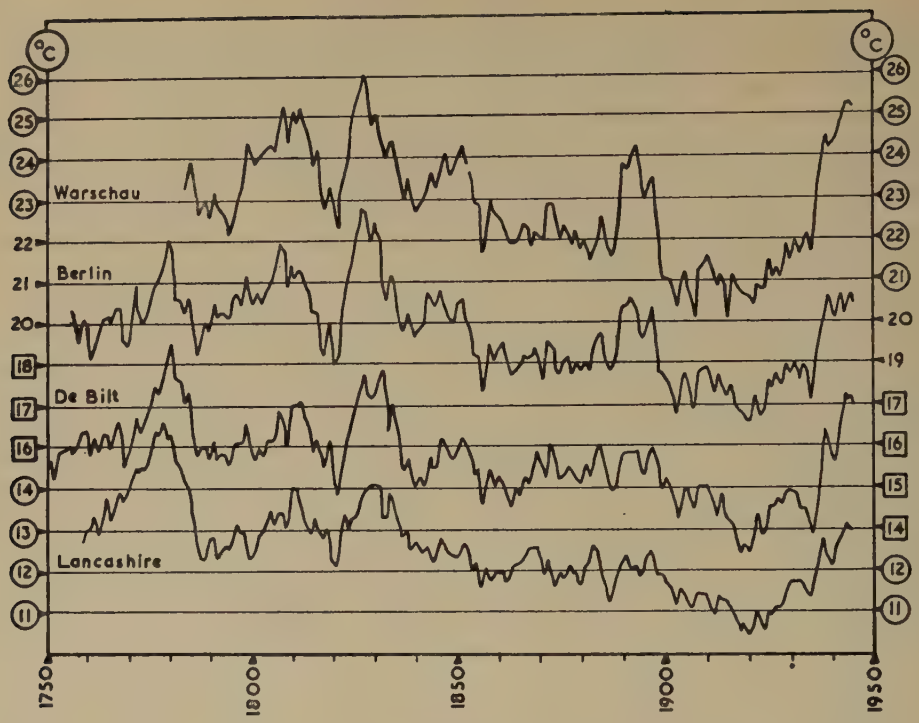

Figure 6. Ten-year running means of the amplitude of average temperature January-July (after Labrijn, by courtesy of *Hemel en Dampkring*).

must be admitted that the reduction of early temperatures is extremely slow and troublesome and the risks of error are large. The possibility of standardizing the early instrumental means in marginal climates against the frequency of snow or the thickness of ice has been considered by the present writer (Manley 1952).

The nature of the variations for individual months, smoothed to decadal running means, can be observed in this *Journal* (Manley 1949, also Fig. 2 above). Similar diagrams have been drawn by numerous other writers, for example Eythórsson (1949). Perusal of a number of these, e.g., those in Willett's paper in the *Centenary Proceedings* (1950a) will undoubtedly bring to mind the fluctuating manner of flow of the turbulent river previously suggested as a model for discussion. Similar curves smoothed to 30-year running means are given by Lysgaard (1949).

Statisticians have devoted some attention to the long-period records, and have at times scrutizined the appearance of the earlier data. With regard, for example, to the New Haven record, the longest (1780 onward) in America, Landsberg (1949) thinks that it is acceptable.

The cumulative impression is that over northern Europe (Fig. 6) the range of temperature January-July has decreased considerably since the late 18th century, with a less marked peak about 1850, and a decided rise since 1930. Such fluctuations certainly lend themselves to explanation in terms of variation of the zonal index. There has been a decided rise of winter temperature since 1850 which has become more pronounced and widespread since 1885 (Willett 1950a). According to him, ' it is most pronounced in the higher middle and particularly the polar latitudes of the northern hemisphere.' It is to be noted that the dates of the most forward positions of the snouts of Icelandic glaciers are put by Thorarinsson (1943) about 1750 and 1850 with a less marked advance culminating about 1890; in western Canada (Field 1949, Lawrence 1950, Mathews 1951) there

is evidence for advance to the 1890's; in Patagonia (Nichols and Miller 1951) up to some date between 1870-1880. Such correspondences are noteworthy.

Throughout all the curves of 10-year running means of temperature there is a general impression that there are minor fluctuations of irregular length which however average between 20 and 25 years (varying, on a quick sample, from 12 to 30). The present amelioration since 1925 is presumably of this order of size; we do not yet know, but we may already suspect (cf., Liljequist 1949, Rodewald 1952) that it is on the wane.

Further smoothing of the curves to 60- or 90-year running means reduces the amplitude of variation but does not by any means eliminate it; and Lysgaard indicates the possibility of an overall variation of the sunspot number over a period of about 80 years. Whatever be the nature of the variations in the atmospheric circulation associated with such fluctuations of solar activity (a point discussed by Willett, *Geogr. Ann.*, p. 186, 1950b), the possibility of a climatic fluctuation of this order (90 to 110 years) has not escaped note by other workers (Easton, Köppen, Mèmèry among others, cited by Flohn, p. 355, 1950b).

On account of the small variations in the mean temperature which are involved, to reach decisions with regard to such fluctuations over the centuries before 1700 is not easy. That the 17th century in England could have differed little from the present is well established (Baker 1932), and we have but scanty evidence before that. For central Europe however Flohn in the above paper has assembled a good deal of evidence which can be taken to indicate that the period 1250-1700 was characterized by a number of minor fluctuations of length and amplitude similar to those of the ' instrumental ' period. He draws attention for example to the evidence for generally mild winters 1420-28 followed by a sharply colder group through the period 1429-1443, and a series of cooler summers 1451-1464. Interest in such fluctuations has developed in Germany; in a recent paper Richter (1952) discusses the evidence for abandonment of various types of land by peasant farmers. She concludes that from 1360-1430 dry sandy soils tended to become waste, whereas from 1430 onward wet and clayey soils were abandoned, testifying to a wet cool epoch which agrees with Flohn's evidence from the sources which indicate a tendency for cool springs and ' monsoonal summers.' It is obviously easy to correlate such phases with a greater and more vigorous penetration of the ' European monsoon ' – that penetration of Atlantic air from late June onward which is characteristic of the more disturbed and rainy Continental summers, and which can again be equated with Willett's ' high-index ' circulation. Likewise, the same tendencies recurred with greater emphasis after 1550; and by 1600 the widespread and noteworthy glacier advances in the Alps begin to be recorded. On the northern margin of European settlement in Iceland, historical records and glacier fluctuations have been studied by Thorarinsson (1943, 1944) aided by a chronology based on the occurrence of ash layers from dated volcanic eruptions. Reasonable estimates of the variation of the summer mean temperatures since 1000 A.D. can now be made, and should be capable of relationship to other events in Europe.

The assemblage of scattered historical data of this kind must however be slow. Further all the evidence so far goes to show that these minor fluctuations of the twenty-five-year order are likely to have occurred throughout historical and post-glacial times.

Fluctuations of climate of this first order can also be studied in North America from the New Haven record which runs back to 1780. There are also others running back for upwards of 100 years, for example, New York and Toronto. There is also some evidence of fluctuation of like order in the tree-rings (Schulman 1951). Schulman's article should here be cited : ' The cycles found in tree-growth seem to be characterized by variability in length, amplitude and form, to tend to appear and disappear according to no discernible law, and to occur in almost any combination. A satisfactory physical explanation of these characteristics has not yet been developed.'

Granted that there exist recognizable fluctuations of mean temperature, and of the January-July range, over periods of irregular length but broadly of the order of 20-25 years based on running means, we have to ask why upon these irregular fluctuations others appear to be superimposed. The evidence of marine organisms records marked variations in the prevailing temperature of the ocean surface, which may in some areas lag considerably behind the variations in the temperature of the air, owing to the input of melt-water for example derived from sea-ice or from glaciers. Decisions with regard to the capacity for variation of the temperature of the ocean surface must await the findings from the core-samples taken at great depths in the ocean (Ovey 1950). These promise to give extraordinarily interesting results, both in regard to water temperature and in regard to dating. The question remains open, however, as to the degree of precision with which past surface temperatures can thus be determined. More recently the development of the oxygen-isotope method promises to give results of the greatest value rather more quickly than by the other methods (cf., Wiseman and Ovey 1950). Even then, it is not at present considered that the temperature of the water in which an organism grew can be given within about 1°F to 2°F, partly because of the fact that not all organisms live at the surface itself and the temperature gradient in the first few metres may be considerable.

At present, therefore, the position can be summed up in these terms :

In North-West Europe

(1) Studies of climatic variations of the second order, since the Climatic Optimum and to some extent before it, point to fluctuations operative over periods of the order of one to three centuries with an amplitude, as regards the mean annual temperature, of about 2°F. This amplitude cannot, however, be expressed exactly. Core-samples from the ocean and from deep lakes appear likely to give much-needed data.

(2) Studies of variations of the first order indicate that fluctuations exist with an amplitude of the order of 1°F operative over irregular periods of about 22 ± 10 years.

(3) The latter can be related to the former; both appear to be relatable to longer-term fluctuations of the zonal index of the circulation. As the former amplitude cannot be expressed at all exactly it is not yet possible to say how much might be ascribed to the zonal-index and how much to variations in the surface temperature of the North Atlantic.

(4) Inasmuch as, in Late-Glacial times, fluctuations of this second order of magnitude appear to have taken place with similar effects although the sea temperature was 10°F lower, we need to know much more about the way in which the sea-surface temperature has varied; core-sampling methods may in time provide the evidence.

(5) Our dating methods do not yet allow any close estimate of the time at which various climatic conditions prevailed; and even our mean summer temperature estimates before 1700 may be subject to errors of a degree or more, which implies that we cannot as yet place in proper proportion the second-order and first-order fluctuations with regard either to amplitude or duration.

(6) In North America studies on similar lines have been made, especially in regard to the glaciers, vegetation and sea temperatures in the west, from Alaska to California; they cannot yet be confidently correlated in time with those of Europe. Accordingly, progress since 1921 has led to greater precision, but we still need much field work. The relationships of the first-order fluctuations over the earth have been studied by Lysgaard, and they do not suggest the view that there has in the past 30 years been a world-wide amelioration; this is now borne out by the temperature gradients taken in borings in the Antarctic ice by the recent Norwegian-Swedish-British expedition (Roots 1952). Recent fluctuations in regard to temperature are appreciably more marked round the North

Atlantic Ocean, and have led to studies of the variation in sea-surface temperature, of resultant wind direction, and of the extent of ice (Eriksson 1943, Smed 1948, Brown 1953).

Moreover, we must not be tempted to assume that climatic fluctuations of the second order must necessarily have a greater amplitude than those of the first order. Willett (1949b) was inclined to the view that they should; Faegri (1950), on the other hand, thinks not; as a botanist he considers that the longer such a fluctuation lasts, the more complete is the reaction of all indicators. He doubts if any of the Quaternary climatic fluctuations have had a greater amplitude than the difference between individual years. But we cannot settle this without closer dating and closer temperature determinations. It is noteworthy that for any time before 1700 it is difficult to bring the estimates of summer mean temperature, for example, for any region in NW. Europe, within 1°C or, say, 2°F; and this applies to first-order and second-order fluctuations alike.

It may further be asked why there has been an overall decline of mean temperature since the Climatic Optimum, shown by the fact that the temperature has never since attained those values for long enough to affect the vegetation on a widespread scale. Here it appears best to invoke for NW. Europe long-term changes in the surface temperature of the North Atlantic. These, while apparently not affecting the intermittent changes in the vigour and character of the circulation, do bring down the overall pitch about which the minor fluctuations occur. It is otherwise very difficult to see why a number of changes, small in themselves, have resulted in a certain amount of recession of vegetation and increased development of glaciers, although the changes have not apparently been greater in magnitude than we experience today between groups of good and bad summers. As we have seen however there is some reason to suppose that we passed the peak of an interglacial period between 5,000 and 7,000 years ago; (cf., von Post 1946, Conway 1948) and this is probably part of a third-order fluctuation.

The magnitude of such changes in temperature and their dating in relation to the occurrence of other phenomena appear likely to become known at no distant date from the deep ocean cores of which preliminary studies have already been published. Such studies may also settle the long-disputed question of the significance, if any, of changes in the latitudinal intensity of the radiation received at the Earth's surface arising from the perturbations of the earth's motion, which have continued to receive support as an important factor in the development of ice-ages (Zeuner 1945) though against considerable criticism (Simpson 1940, Charlesworth 1953) on grounds of effectiveness and for other reasons. Further studies of the extent of the variation of the temperature of the ocean surface waters in equatorial latitudes will also be eagerly awaited; some indeed can be expected in the near future. That fascinating relationships are likely to be found is already apparent from Wiseman's work (1951a) on the CO_2 content at various horizons in an equatorial-Atlantic core, collected at a locality where there are few variables and few components. In a diagram (unpublished) to which he allows me to refer, he has illustrated the remarkable agreement between the fluctuations of surface water temperature, deduced from that core, over the last 15,000 years, and the present writer's estimates of the summer mean temperature at Windermere; this last ought of course to be associated with that of the North Atlantic west of Ireland. Such relationships, if real, will indeed be significant (cf., Wiseman 1952b). Clearly we may look for great advances when, for example, we have acquired knowledge of the distribution of surface water temperature throughout the North Atlantic at an ice-age maximum.

Climatic variations of higher or 'geological' order

Of these variations, for which the evidence is overwhelmingly geological, little need be said. To the resumé by Dr. C. E. P. Brooks we need only for the moment add that

pollen-analytical studies have now been extended to interglacial deposits. These are well illustrated in a very recent paper (Hollingworth, Allison and Godwin 1950) in which deposits at Cambridge are discussed which appear to show for part of the Riss-Würm interglacial pronouncedly oceanic conditions, through the high proportion of ivy pollen; further references are given. It is to be observed that in NW. Europe the interglacials are now known to provide a sequence of vegetational development resembling in essentials that which has occurred in the post-glacial, but including the full cycle of change from sub-arctic tundra through deciduous warm-temperate forest and back to tundra at the onset of the next glaciation. Accordingly, we are again led to the view (von Post 1946) that we are now living in the later stages of an interglacial period in which the optimum warmth was passed upwards of 5,000 years ago. The possibility that in Greenland the initial stages of our present Ice Age began much earlier than the Pleistocene was indicated by Wager (1933). References to interglacial studies are given by Zeuner (1949).

The recent development by Urey and his colleagues (1948) of the oxygen-isotope method of determining the temperature of the medium in which organisms grew appears capable of indefinite extension. Fossils of an age upwards of 100 million years have been so analysed and we may accordingly expect an abundance of data with regard to the closer definition of the temperature of the seas in which our older sediments were laid down. The method, moreover, can be extended to lakes and perhaps to other substances besides the carbonates. There are also signs that other effective geo-chemical methods under development will be more widely known and used (Wiseman and Ovey 1950).

Conclusion

Sufficient has been said to show that the present is a period in which abundance of evidence is being sifted and interpreted, and that evidence once acquired can be fitted into a frame with increasing ease and satisfaction. Yet if, even in NW. Europe, we are in difficulties with regard to the range of mean temperature and the timing of fluctuations, it is evident that over the rest of the world we are very far from being able to discuss such questions as the synchronization or otherwise of changes in the two hemispheres. Pollen-analytical studies in New Zealand, for example, have been assumed to show fluctuations of the same type, occurring at about the same epoch, as those in the N. Atlantic region (Raeside 1948); but we need closer dating before such a fact can be firmly established. Until that can be done, we must be cautious in assuming that fluctuations in the Atlantic region are necessarily accompanied by events elsewhere of sufficient magnitude to affect vegetation, however much we may be tempted *a priori* from our present ideas of the general circulation. This is the significance of Lysgaard's extended discussion (1949); the recent fluctuation, which we may consider first-order, is neither uniform nor world-wide in its effects, and we may regard this as supporting the view that the shorter the fluctuation the less will be the agreement with fluctuations in progress elsewhere. We may be ready to assume that the second-order fluctuations are more widespread as regards magnitude and consequences; but we must greatly improve our knowledge of the dating of events.

Discussion of the variations of precipitation and evaporation has so far been omitted, inasmuch as studies of temperature fluctuations and their timing appear likely at present to be more productive of results. In equatorial latitudes however where the fluctuations of temperature are in any event likely to be small, studies of the variations of rainfall in regions such as East Africa (Nilsson 1949) are again likely to point to variations in the general circulation, provided that they can be more precisely correlated with events in temperate latitudes. Thus we again face the necessity for more accurate dating; yet from the examination of the equatorial oceanic core-samples now in progress, and perhaps

also those of certain lakes, we may again look forward to marked advances in knowledge in the near future. The field too is open with regard to the past relationships between the fluctuations of the Indian monsoon and events elsewhere.

This outline of the present position with regard to the study of climatic variation has largely been confined to the work on temperature going forward in NW. Europe, for reasons earlier stated. Enormous quantities of evidence are being brought forward; and very slowly, an increasingly firm framework of reference is being provided into which any particular findings from historical, archaeological, botanical, glaciological or geological sources can be put. Progress is being made in North America along similar lines; the numerous papers in the *Bulletin of the American Geological Society* (1949) on Pleistocene research should be referred to. Even in NW. Europe, however, caution is necessary before embarking on too elaborate a development of meteorological theory, in the present state of knowledge of past temperatures and past dates. Impressive and stimulating discussion of the greatest value to meteorologists has arisen as a result of the new evidence provided in recent years, which integrated in such comprehensive papers as those of Ahlmann and von Post, has come at a time when a climatic amelioration has for other reasons attracted widespread attention. So much work is now about to come forward that the present is not the best time to write; but we may confidently expect a new surge of understanding of all problems associated with the general circulation, not merely from the day-to-day studies of the upper air, but also from the integrated findings of a world-wide host of workers in other sciences with regard to the variations of climate over the whole range of the earth's history.

We have long been aware of the several possible causes of climatic variation. The question arises, which are the more important; and that we cannot answer without a better knowledge of the amplitude, length, and correlation of climatic variations of different order over the earth. The present therefore is evidently a time in which greater precision of post-glacial dating is perhaps the first requirement, and this may soon be provided from the ocean floor. More precise determinations of the seasonal range of temperature in the continental interiors will no doubt follow. Lastly, we are increasingly aware of the significance of present events in relation to those of the past; the ' blocking highs ' of 1947, the exceptional warmth of 1949, the absence of maritime-tropical incursions in 1951, the great gales of 1952-53 all provide food for thought with regard to past vicissitudes, towards the elucidation of whatever greater pattern may exist.

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Dr. Ahlmann's 'Bowman Lecture' (International Geographical Congress, Washington) will be published in 1953 and is of the highest value.

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A quantitative analysis of the evolution of large-scale flow with regard to the effect of eddy motion

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Summary

The theory of pressure change by barotropic redistribution of absolute vorticity is extended to include the effect of an eddy motion represented by the departure of the daily value from a running five-day mean. Application is made to a case in which the mean motion at the 500 mb level is featured by quasi-stationary approximately zonal flow over North America and by slowly moving large vortices farther downstream (blocking situation of June-July 1949). One result indicates the potentiality of a technique of forecasting the evolution of mean flow based on a persistence tendency of the ' errors ' in barotropic tendency computations on mean charts. A further result concerns the interpretation of the ' error ' field in the light of the neglected processes. There is, contrary to earlier results, no evidence of standing perturbations due to mountain ranges, but the horizontal eddy transfer of vorticity is shown to be an important factor. It is demonstrated that the latter transfer, viz., by the smaller eddies on approximately the cyclone-wave scale, is mainly directed against the gradient of vorticity in the mean flow, thereby playing an essential role in the maintenance of the larger eddies. The transfer coefficient (diffusivity of relative geostrophic vorticity) is therefore predominantly negative, and its magnitude is from 10^9 cm^2 sec^{-1} to 10^{10} cm^2 sec^{-1}. The action of the individual smaller eddies in transferring mean vorticity is evidenced by their paths in the relevant mean flow patterns.

1. Introduction

The process of turbulent transfer of mean flow properties, whose discovery led to such remarkable advancements in the field of microturbulence, has been recognized as an important one in the general circulation of the atmosphere. In synoptic meteorology too one is restricted to the study of artificially smoothed flow although one loosely refers to it as instantaneous flow. In the free atmosphere this is so not only on account of an instrumental smoothing of microturbulent fluctuations but even as a result of smoothing always done in standard analysis. The physical nature of the fluctuations – within fairly wide limits of space and time – about the synoptically smoothed field of horizontal motion is an open question. If instrumental technique suppresses *ab initio* microturbulent fluctuations, the effect of which is usually disregarded above the ' frictional layer,' synoptic smoothing will necessarily suppress fluctuations of the type of long gravity waves and of certain types of convection cells. In either case, the neglect of the fluctuations on the evolution of the mean field is equivalent to disregarding the Reynolds stresses and hence, ultimately, diffusion of mean properties. It is apt to quote from a recent paper by Richardson and Stommel (1948) :

> Thus Defant found the diffusivity K to be of the order of 10^{11} cm^2 sec^{-1} when cyclones are treated as eddies in the mean circulation of their latitude. Richardson and Proctor showed that Defant's value is not isolated, but the climax of continuous variation from the molecular diffusivity $K = 0{\cdot}2$ cm^2 sec^{-1}.

It is not certain whether in this connection continuity is a concept that can be maintained in the face of synoptic evidence. Recent studies of large-scale flow, e.g., Berggren, Bolin and Rossby (1949), indicate the existence of characteristic peaks in the energy spectrum of motion in the atmosphere. One may express the notion of a corresponding ' spectrocline ' by referring to the distinction between wave motions of different synoptic scale : long waves and cyclone waves. There is little doubt that the

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disturbances interact strongly and that this interaction will be of particular importance for the maintenance of those large-scale, quasi-stationary eddies observed in blocking situations. Theoretical aspects of this problem have been considered, partly in relation to critical flow analogous to a hydraulic jump (Rossby 1950), and partly in relation to selective wave-instability (Eady 1949).

The present study treats of the behaviour and mutual dependence of varied-scale component flows that result from smoothing with respect to time, viz., from averaging the synoptically smoothed field over a number of days. Turbulence being a compensation for smoothing, its properties will largely depend on the choice of the averaging interval. Five days was considered the most suitable choice with a view to separating component motions on the long-wave and the cyclone-wave scale respectively. The eddy motion was then that which resulted from the fluctuations of the synoptically smoothed value about a running five-day mean.

2. The barotropic tendency equation for mean flow and the interpretation of the eddy-transfer term

For the purpose of deriving the barotropic tendency equation of mean horizontal motion it is useful to start from the vorticity equation for horizontal flow in continuity-equation form,

$$\frac{\partial \zeta}{\partial t} = -\nabla \cdot (\eta V) \qquad (1)$$

where ζ is the vertical component of relative vorticity, η the vertical component of absolute vorticity ($\eta = \zeta + f$, f being the Coriolis parameter) and V, ∇ denote the horizontal velocity vector and gradient respectively. The local change of vorticity given by Eq. (1), by the assumption of barotropy, implies the absence of solenoidal and frictional sources of vorticity and disregards the effects of vertical motion. With these assumptions, however, it is permissible to neglect the horizontal divergence $\nabla \cdot V$ as a small quantity. The horizontal convergence of the flux of absolute vorticity, represented by the right-hand side of Eq. (1) reduces then to the horizontal advection of vorticity and we obtain the well-known barotropic vorticity equation

$$\frac{\partial \zeta}{\partial t} = -V \cdot \nabla \eta \quad \text{or} \quad \frac{d\eta}{dt} = 0 \qquad (2)$$

expressing the invariance of η. With a view to keeping in mind the general nature of the problem in hand, it will, however, be not inappropriate to refer to the advection term as a differential vorticity flux (reduced by the assumption of non-divergence). Using the geostrophic-wind equation for the evaluation of V and ζ, Eq. (2) may be written (see Bolin and Charney 1951)

$$\nabla^2 \left(\frac{\partial z}{\partial t}\right) = J(\eta, z) = J\left\{\left(\frac{gm^2}{f}\nabla^2 z + f\right), z\right\} \qquad (3)$$

where z denotes the height of the pressure surface that most closely coincides with the mean level of non-divergence, taken in the computations below as the 500 mb surface. J is the Jacobian operator, g the acceleration of gravity and m the scale factor of the transformation from spherical coordinates to the particular map used in the computations.

We proceed to transform Eq. (3) for mean motion. The analogue corresponding to Reynolds equation of motion is derived by introducing

$$z_i = \bar{z} + z_i^* \qquad (4)$$

where $\bar{z}$ denotes the average of z_i over an interval covering n equally-spaced observed z_i ($i = 1, 2, \ldots n$) and z_i^* the eddy fluctuation, i.e., in practical application the difference between the value z_i taken once or twice daily from analysed maps and the constant mean $\bar{z}$ ($= \frac{1}{n} \sum_{i=1}^{n} z_i$). Averaging each term and substituting, Eq. (3) takes the form

$$\nabla^2 \frac{\partial \bar{z}}{\partial t} = J(\bar{\eta}, \bar{z}) + \overline{J(\eta_i^*, z_i^*)} \quad . \quad . \quad . \quad . \quad (5)$$

In this study it is not possible to adhere to the artifice of a constant mean $\bar{z}$, since the variation of the five-day mean is one of the quantities which has to be considered. Instead of Eq. (4) we have to use

$$z_i = \bar{z}_i + z_i' \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where

$$\bar{z}_i = \frac{1}{j} \sum_{j=i-(n-1)/2}^{i+(n-1)/2} z_j \quad . \quad . \quad . \quad . \quad (7)$$

is a running mean value and is varying quasi-continuously (for brevity's sake the subscript i will be dropped henceforth), z' is thus the eddy fluctuation defined as the departure of z from the running mean. It has been pointed out by Kampé de Fériet (1951) that $\bar{z} = \bar{\bar{z}}$, and thus $\bar{z}' = 0$, only in the case when $\bar{z}$ is a linear function of time, which restriction includes of course the previous trivial case of no change in time. Therefore when introducing (6) in (3) the terms $J(\bar{\zeta}', \bar{z})$ and $J(\zeta, \bar{z}')$ as well as $\overline{\nabla^2 \partial z'/\partial t}$ will not in general vanish identically. In view of the rather slow variation of the running mean in the present study we shall assume that the terms are in general small in comparison with $\overline{J(\eta', z')}$ deriving from the differential eddy flux $\overline{V' \cdot \nabla \eta'}$. With this assumption the tendency of the running mean is determined by the equation

$$\frac{\partial \bar{z}}{\partial t} = \frac{1}{\nabla^2} [J(\bar{\eta}, \bar{z}) + \overline{J(\eta', z')}] \quad . \quad . \quad . \quad (8)$$

where dividing by the Laplacian operator ∇^2 indicates symbolically the procedure of solving a differential equation of the type Eq. (3) by standard relaxation methods.

For the general purpose of developing an adequate technique for numerical prediction of mean flow one may express the second term on the right-hand side of Eq. (8) by mean motion quantities and a transfer coefficient K, viz., the eddy diffusivity of vorticity. For absolute vorticity the equation of diffusion may be written as

$$\frac{d\bar{\eta}}{dt} = K \nabla^2 \eta \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In virtue of Eq. (2), $d\bar{\eta}/dt = -\overline{d\eta'/dt}$ and since the quantity $\overline{\partial \zeta'/\partial t}$ vanishes we have

$$-\frac{\overline{d\eta'}}{dt} = -\overline{V' \cdot \nabla \eta'} = K \nabla^2 \bar{\eta} \quad . \quad . \quad . \quad . \quad (10)$$

It is at this stage appropriate to introduce $(\partial \bar{z}/\partial t)_e$, viz., the contribution of the eddy transfer $-\overline{V' \nabla \eta'}$ to the rate of change of the mean contour height. Again using the geostrophic assumption it follows from Eq. (10)

$$\frac{g}{f} \nabla^2 \left(\frac{\partial \bar{z}}{\partial t} \right)_e = K \left\{ \nabla^2 \left(\frac{g}{f} \nabla^2 \bar{z} \right) + \frac{\partial^2 f}{\partial y^2} \right\} \quad . \quad . \quad . \quad (11)$$

It is clearly of no advantage to consider here a variability of the transfer coefficient K in the constant-pressure surface; it will be assumed constant as a first approximation.

Disregarding furthermore terms containing $\partial^2 f/\partial y^2$, viz., the variation with latitude of Rossby's parameter β – which simplification amounts to introducing a certain distortion of the earth's vorticity field – we obtain the particular solution

$$\left(\frac{\partial \bar{z}}{\partial t}\right)_e = K \nabla^2 \bar{z} \quad . \quad . \quad . \quad . \quad (12)$$

This equation may be regarded as determining the diffusion of geopotential, $g\bar{z}$, since for all practical purposes g is a constant. Positive K would mean equalization of potential and a reduction in course of time of geostrophic motion, negative K would be associated with increasing potential gradient and increasing geostrophic (mean) motion. The changes in the motion cannot however vary the mean kinetic energy over a sufficiently large region since we have assumed barotropic redistribution of vorticity.

Following the classical theory of turbulence, eddy diffusivity may be expressed in terms of mixing length and eddy velocity. The variability of the mean motion introduces, however, serious complications. It is difficult to develop the theory of eddy transfer in these circumstances and one must stop at an examination of the validity of Eq. (12). Such examination was made using for $(\partial \bar{z}/\partial t)_e$ the value obtained from the distribution of the eddy Jacobian and from the subsequent relaxation of the field of $\nabla^2 (\partial \bar{z}/\partial t)_e$ (see Section 4).

3. Response of five-day mean 500 mb flow to barotropic vorticity redistribution in a summer blocking case

(a) *Computed and observed tendencies* (x_1, x_2)

The method used in the computations of tendencies according to Eq. (8) was similar to that applied at Princeton and at Stockholm to ' instantaneous ' flow (see Bolin and Charney 1951). They were made on a rectangular grid with mesh size 600 km at 50°, viz., approximately double the size used at those places. The choice was regarded as appropriate for the computations and the relaxations of the main Jacobians and was used by way of experiment also for the eddy Jacobians.

The area of the evaluated quantities covered Europe; the North Atlantic, north of about 38°N.; Greenland and most of Canada and the U.S.A., east of about 85°W. The area in which the daily 500-mb contour heights were extracted was accordingly larger, viz., by double the width of the mesh size added to the four boundaries of the former. The eddy fluctuations in contour height and the eddy Jacobians were computed for only slightly smaller areas. In the calculation of the correlation coefficient for computed tendencies and observed changes a further row in the interior was dropped because observed changes were used as boundary conditions in the relaxations. The influence of these conditions affected then the computed tendencies used in the correlations but very little. The data were taken from the 500-mb charts published by the Deutsche Wetterdienst, U.S. Zone, Bad Kissingen (1949). In exceptional cases and over limited areas only were changes made in analysis.

The method of verification of the computed tendencies was mainly that used by Bolin and Charney (1951). Thus $\int_t^{t+\Delta t} \tau dt$ was replaced by $x_1 \equiv \tau_m \Delta t = \frac{1}{2}(\tau_t + \tau_{t+\Delta t})\Delta t$ for comparison with observed changes $x_2 \equiv \bar{z}_{t+\Delta t} - \bar{z}_t$, where τ denotes the tendency $\partial \bar{z}/\partial t$. The verification interval Δt in the earlier tests on daily or twice-daily charts was taken as 12 hr or 24 hr. It is obvious that similar trials on five-day means have a meaning only if that interval is increased at approximately the ratio of the relevant averaging intervals. The interval was here taken as three days, but special tests with shorter and longer intervals have been made too.

The computations were made in a case of pronounced blocking of westerly flow over the eastern Atlantic and Europe concurring with markedly persistent though gradually decreasing nearly zonal flow over U.S.A. and southern Canada, between 16 June and 5 July 1949. (Compare the 500-mb contour charts Fig. 5). This case had been earlier investigated by Rex (1950), who studied among other things the geometry of the flow in an attempt to verify the above-mentioned analogue to a hydraulic jump, based on the dynamic principle of a (critical) minimum momentum-transfer in the general eastward direction of the flow.

We shall approach the general problem of maintenance of the large-scale eddies under the influence of vorticity transfer by the smaller-scale eddies in a later section. Here we shall be chiefly interested in the extent to which the mean flow at the 500-mb level is controlled by vorticity transfer through its own streaming or, in other words, by a (barotropic) redistribution of its own vorticity.

TABLE 1. THE LINEAR CORRELATION PARAMETERS FOR COMPUTED (MEAN) BAROTROPIC TENDENCIES AND OBSERVED CHANGES OF FIVE-DAY MEAN CONTOUR HEIGHT (x_1, x_2). For last column see the text ; verification interval Δt is 72 hr. Number of correlates is 55.

Pair of five-day periods June 1949		$r(x_1, x_2)$	a	b	$r(x_c, x_2)$
16-20	19-23	0·60	0·8	2·2	0·75
		0·68	**0·9**	**1·8**	-
19-23	22-26*	0·32	1·4	1·7	0·62
		0·37	**1·5**	**1·4**	-
22-26	25-29	0·38	1·0	− 0·9	0·76
25-29	28-2 July	0·45	1·0	− 1·2	0·63
Average		0·44	1·1	0·5	0·69
* Omitting four adjacent gridpoints in the Newfoundland area the values in the first three columns for this test are		0·55	1·0	0·6	
		0·58	1·1	0·3	

Table 1 gives the linear coefficient of correlation r for x_1 and x_2, the regression coefficient a and the constant b in the regression equation $x_1 = ax_2 + b$. The values of r are definitely below the threshold of usefulness in forecasting for three days and no physical significance should be readily ascribed to the simultaneous regression coefficient a (here close to unity) and to the constant b. The figures in bold type in Table 1 show the corresponding values in the first two tests when the tendency was computed from Eq. (8) *with* the inclusion of the eddy Jacobian. The correlation coefficients are slightly higher. It is well known that the rate of improvement of such coefficients does not always bring out the general significance of the modification of one of the correlates, x_1 in this case. One of the reasons for this is the presence of an internal correlation between the samples of the correlates themselves.

TABLE 2. LINEAR CORRELATION PARAMETERS FOR CENTRED BAROTROPIC TENDENCIES AND OBSERVED CHANGES OF RUNNING FIVE-DAY MEAN CONTOUR HEIGHT, FOR DIFFERENT VERIFICATION INTERVALS Δt. Computed tendencies centred on period 19-23 June, 1949. Number of correlates is 51, the same four adjacent points as in footnote to Table 1 having been omitted.

Δt (hr)	$r(x_{1\cdot 0}, x_2)$	a	b
48	0·59	0·94	1·8
96	0·64	1·42	0·8
144	0·58	2·80	− 0·9
Average	0·60	1·29	0·6

In order to find out to what extent the length of the verification interval Δt (three days) might be responsible for the low values of r computations with different Δt were made in one case. For this purpose *centred* computed tendencies x_1 were compared with the observed change $x_2 = \bar{z}_{t+\Delta t/2} - \bar{z}_{t-\Delta t/2}$. In this test t was taken as the centre day of the period 19-23 June. The results are given in Table 2. It is seen that raising Δt from 48 hr to 96 hr and from 96 hr to 144 hr did not appreciably affect the correlation coefficient. It is noteworthy that although r does not alter, the regression coefficient increases very markedly with the length of the verification interval. It is unity for about 60 hours in this particular test.

(b) *The persistence of ' errors ' $(x_1 - x_2)$*

Fig. 1 shows the geographical distribution of the average ' error,' $x_1 - x_2 \equiv D$, say, obtained from the four tests in Table 1. This average is denoted by D_m, the individual D referring to computations of x_1 *without* regard to the eddy term. We shall not go here into the distribution of D in the four tests; it suffices to state that these showed a striking degree of similarity although there were some differences in the absolute values and positions of the patterns.

The ' error,' as defined above, may be regarded in the first instance as a consequence of the defect of the barotropic model and the additional assumptions and approximations set out earlier. The terms neglected in Eq. (8) are – apart from the eddy term – chiefly the divergence term and the solenoid term associated with sinks and sources of vorticity due to non-adiabatic processes. Whatever kind of processes might be reflected in the distribution of D_m it is, in slowly varying mean flow, of importance that they are largely taking place in regions fixed relative to the surface of the earth. We are then obviously dealing with perturbation phenomena in the sense that the barotropic redistribution of vorticity is disturbed persistently and in the same direction over such regions. Whether they are topographic waves, vorticity sources due to heating and cooling or such baroclinic developments as are associated with release of potential energy, it is of practical importance to find a means of allowing for these processes in computing the changes of large-scale flow patterns on the basis of a barotropic model. In principle if the quantity D may serve as a means to this end, it will be possible to put barotropic tendency computations on mean charts to better use.

The following computations show to what extent the average error D_m may be applied as a constant correction to individual tendency computations. In each of the four tests of Table 1 the computed change x_1 at each grid point was corrected by subtracting the value of D_m at the same point. Let the corrected three-day change, $x_1 - D_m$, be denoted by x_c. The correlation coefficient for x_c and x_2 was computed for each of those four tests. The result is set out in the fourth column of Table 1. The pronounced improvement of $r(x_1, x_c)$ on $r(x_1, x_2)$ and the rather small differences of $r(x_1, x_c)$ in the several tests exemplify the persistence, in position and magnitude, of the errors D. The climatological aspects of such quasi-stationary fields have been discussed and put to test by Mr. P. F. Clapp (1952b) with whom the author had many fruitful discussions on an occasion of co-operative research at the Institute of Meteorology, University of Stockholm. It may here suffice to point out the potentialities of a technique of predicting the evolution of mean flow based on an appropriate usage of ' past errors ' in barotropic tendency computations.

(c) *Impression of faulty analyses*

To ascertain that the distribution of D, and for that matter of D_m, did not only reflect the degree of reliability, as between different regions, of the analyses of the individual

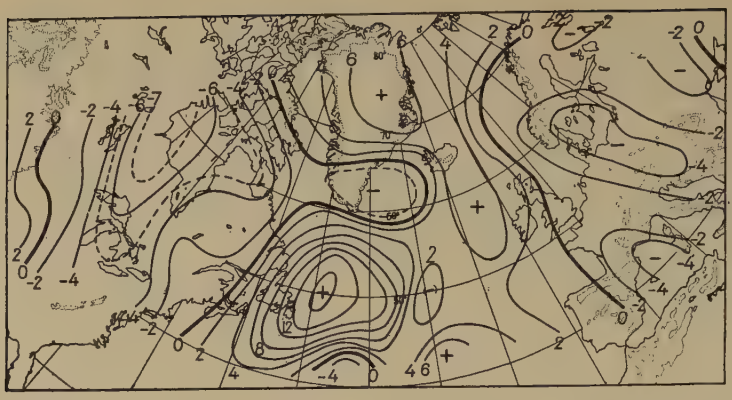


Figure 1. Geographical distribution of the average ' error ' D_m during the period 16 June to 2 July 1949. Numbers denote 72-hr change in decametres.

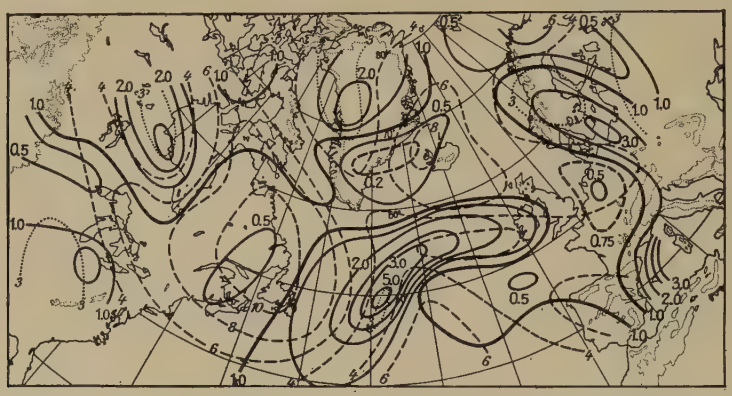


Figure 2. Distribution of the verification index M (full lines) and variability W (dashed line) in period .16 to 29 June, 1949.

contour charts and thus of the $\bar{z}$ at the grid points, a verification index (M) was computed and its geographical distribution mapped. It was considered appropriate to define M as the ratio of the absolute value of D_m to the average absolute value of the observed three-day height variation, the latter being a convenient measure of the variability (W) of the mean contour field : thus $M = |D_m|/W = |x_1 - x_2|_m/|x_2|_m$. Root mean squares are generally more appropriate than algebraic means, but as only four samples, corresponding to the four test periods, were available the distinction is unimportant.

Regions of moderate and high verification were those in which $M < 1$ and $M < 0{\cdot}5$ respectively; remaining regions with $M \geqslant 1{\cdot}0$ were regions of definite failure. Fig. 2 gives the distribution of M and of the variability W of the pressure field. The following points should be noted :

(i) Regions known for an inadequate network of observations coincide *by and large* with regions of failure, as for example, over large parts of the North Atlantic, Canada, especially in the regions of Hudson Bay, Northern Greenland and the small part of the Western Mediterranean covered by the computations. The mean value of M is 1·52 over the Atlantic, 1·16 over Greenland and Canada. On the other hand, regions known for rather good and reliable networks coincide roughly with areas of moderate verification. This concerns especially the U.S.A., and, by and large, also the European area, the mean values being 0·84 and 0·98 respectively.

(ii) There are notable exceptions from the features recorded in (i) above. Labrador, the southern seaboard of Greenland, the Bay of Biscay and the adjacent oceanic region to the westward and finally parts of Finland, Lapland and the adjacent sea to the northward, are areas of moderate to high verification. These areas must be regarded as inadequately covered by reliable observational data.

(iii) Remarkably strong gradients in the M field are apparent in the zones of transition between areas of low and high verification over mid-Atlantic, Greenland and elsewhere.

Weighing the evidence, it is safe to conclude that the effect of faulty analyses – inasmuch as they result from the defects and lack of observational data – on the values of x_1 and x_2 and hence on the quantities M, W and D_m, is but *broadly impressed on the area as a whole*. Consequently an analysis of the D_m field in terms of the neglected processes will be permissible.

(*d*) *Interpretation of 'errors' as effects of divergence, solenoidal source and eddy transfer of vorticity*

The processes just referred to may be expressed in the following relationship for the quantity D:

$$\frac{f}{g}\frac{d\bar{\eta}}{dt} = \nabla^2(x_2 - x_1) = -\nabla^2 D = -\frac{f^2}{g}\left[\left(1+\frac{\bar{\zeta}}{f}\right)\mathrm{div}_2\,\bar{V} - \bar{V}\cdot\nabla\ln\bar{T}\right] + \nabla^2\left(\frac{\partial\bar{z}}{\partial t}\right)_e \quad . \quad . \quad (14)$$

where T is the absolute temperature. In the solenoidal vorticity source the geostrophic wind has been substituted so that it is equal to $fV\cdot\nabla\ln\bar{T}$. For quasi-stationary conditions $\left(D \simeq x_1, \frac{\partial\bar{T}}{\partial t} \simeq 0\right)$ and, neglecting vertical motion terms, it is readily shown with the aid of the first law of thermodynamics and the continuity requirement that the term in square brackets on the right-hand side above becomes $(1 + \zeta f^{-1} + \alpha)\,\mathrm{div}_2\,\bar{V} - (c_v\,\bar{T})^{-1}\,dQ/dt$ where $\alpha = (c_p - c_v)\,c_p^{-1}$, in the usual notation, and dQ/dt is the heat supplied to unit volume in unit time.

TABLE 3. INTERPRETATION OF THE DISTRIBUTION OF THE ERROR D IN TERMS OF THE NEGLECTED PROCESSES. In the first, fourth and fifth columns positive (negative) values strictly apply at and in the vicinity of maxima (minima) within areas of positive (negative) values.

D	$\mathrm{div}_2\,V$	$V\cdot\nabla T$	$(\partial z/\partial t)_e$ or E	$D \simeq x$	dQ/dT
> 0	convergence	cold air advection	< 0	> 0	heat source
< 0	divergence	warm air advection	> 0	< 0	heat sink

An interpretation of the principal positive and negative areas of D_m as the result of *one* of the three processes, horizontal divergence, horizontal temperature advection (or heat supply in the stationary case) and eddy transfer, will be facilitated by the survey set out in Table 3.

A study of Figs. 1 and 3 leads to the following conclusions:

(i) there is no evidence that topographically-produced wave perturbations dominated the D_m field in any area; e.g., the negative areas with marked minima in the lee of mountain barriers such as the Rocky Mountains and the Scandinavian Range would correspond to divergence whereas the topographic effect would require convergence, as is well known. This is in factual agreement with results obtained by Bolin and Charney but reference is made to correspondence on this point (Berson; Bolin and Charney 1953).

(ii) for large-scale systems the solenoid term is known to be small in comparison with other terms in the equation. No estimate of the magnitude of $V \cdot \nabla \ln \bar{T}$ has been made in this case, but if the D_m field were taken as largely a consequence of this vorticity source, the sign of D_m in wide areas would be in conflict with what is empirically known of cold and warm air advection in mid-tropospheric flow patterns. Furthermore, in the extensive area of positive D_m centred near Newfoundland, $D_m = x_1$ to a fair approximation so that, as is seen from Table 3, the distribution there would correspond to a heat source. This however is incompatible with a predominantly westerly flow streaming out over the ocean in summer.

(iii) It will be recalled that the term $(\partial \bar{z}/\partial t)_e$ is a contribution to the rate of change of $\bar{z}$ due to barotropic horizontal eddy transfer of vorticity. It had been shown that when this transfer was taken into account in the computation of tendencies, the correlation coefficient increased slightly in comparison with the coefficient for x_1 and x_2 (first two tests in Table 1). This is brought out in a clearer way by comparing the distribution of D_m with the rate of change of contour height as the integrated effect of eddy transfer during a longer period. This change (E) for the period 16-29 June has been evaluated and is given in Fig. 3, as a rate of change in three days to facilitate comparison with Fig. 1. In many regions, in particular in the Newfoundland area referred to above, maxima of D_m are situated rather close to minima of E, and vice versa. This result, as is seen from Table 3, permits us to attribute the error D_m in such regions largely to the effect of eddy motion which had been neglected in the computation of $\overline{V \cdot \nabla \eta}$. It is observed that predominantly $|D_m| > |E|$. It is not unlikely that the choice of a smaller grid mesh in the computation and relaxations of the eddy transfer term would have resulted in somewhat larger absolute values of E.

TABLE 4. DIFFERENTIAL (BAROTROPIC) TRANSFER OF VORTICITY IN BULK (FIRST COLUMN) AND BY THE EDDY MOTION (SECOND COLUMN) AND THE TRANSFER COEFFICIENT K, GIVEN AS SPATIAL MEANS OF N EVALUATIONS IN THE QUASI-STATIONARY LARGE CIRCULATION SYSTEMS. Period is 16 June to 2 July 1949. The figures in the first two columns are in arbitrary units.

	$\overline{V} \cdot \nabla \overline{\eta}$	$\overline{V' \cdot \nabla \eta'}$	K	N
Atlantic cyclone	− 5·8	+ 1·4	− 3·5 × 10⁹	11
European cyclone	− 0·8	+ 1·3	− 3·3 × 10⁹	9
Blocking high	− 3·2	+ 0·2	+ 0·2 × 10⁹	15

In view of the uncertainty regarding the average magnitude of E a direct comparison of the bulk-transfer term $\overline{V} \cdot \nabla \overline{\eta}$ with the eddy-transfer term $\overline{V' \cdot \nabla \eta'}$ in the vorticity equation is indicated. In Table 4 these quantities are given in arbitrary units as mean values for each of the large quasi-stationary eddies downstream from the westerlies over America. The tendency on the part of these transfers to oppose each other is distinct, at least for the cyclonic systems. This brings out the possibility of an important mechanism which could have already been inferred from the evidence listed in (iii) above, viz., *the eddies on a scale below, but comparable with, the scale of the large-scale quasi-stationary cyclones are an agency for maintaining the latter. More generally the maintenance concerns the large-scale flow as a whole in periods featured by marked deviations from the seasonal normal patterns.*

Regarding the quasi-stationary anticyclonic eddy (the blocking high) the relevant figures in Table 4 are in themselves no evidence of that mechanism, since the eddy term is negligibly small in comparison with the bulk term. This result, however, may be explained by the fact that it was the blocking high which underwent a distinct weakening characteristic of the subsequent more rapid decay.

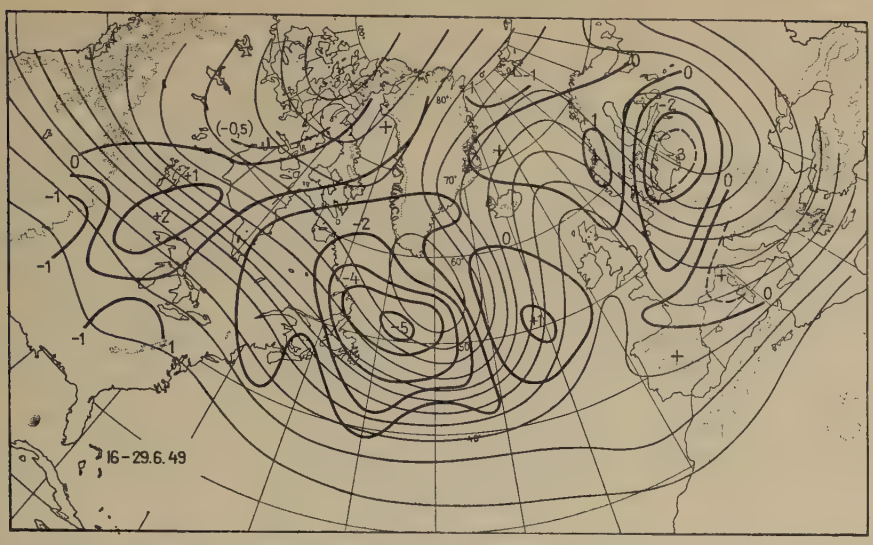


Figure 3. Solid curves show the distribution of the quantity E (eddy contribution to computed change of contour height, in decametres per 72 hr). Thin curves show the mean 500-mb contours. Period is 16 to 29 June, 1949.

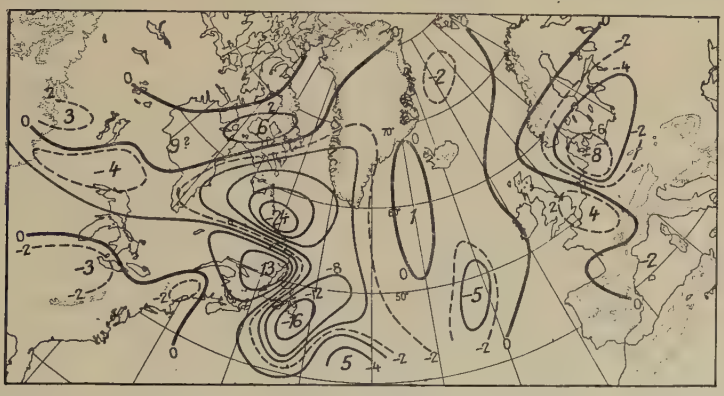
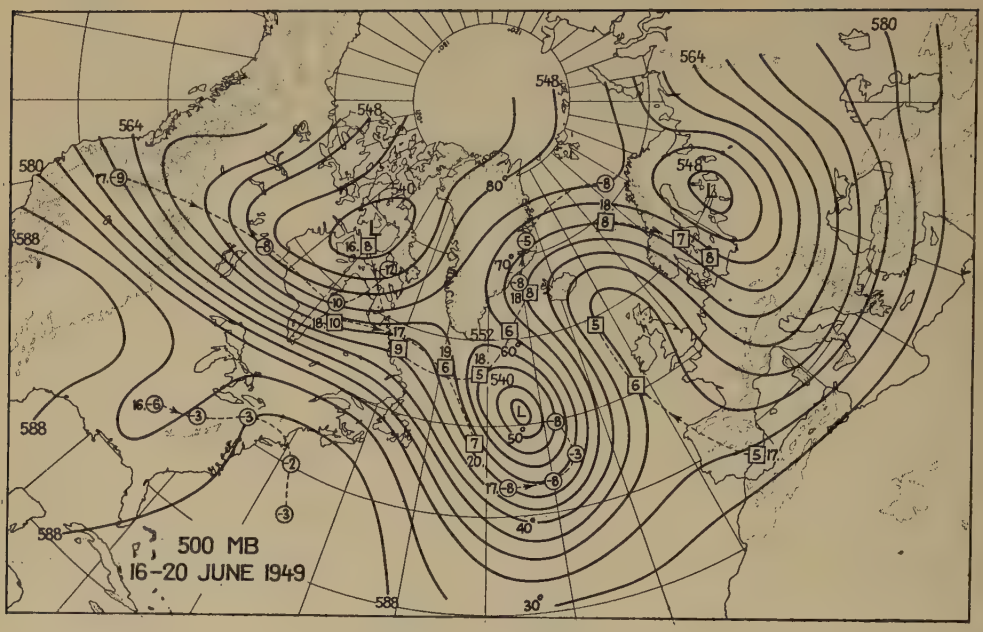


Figure 4. Distribution of the computed eddy transfer coefficient K, unit 10^9 cm^2 sec^{-1}.

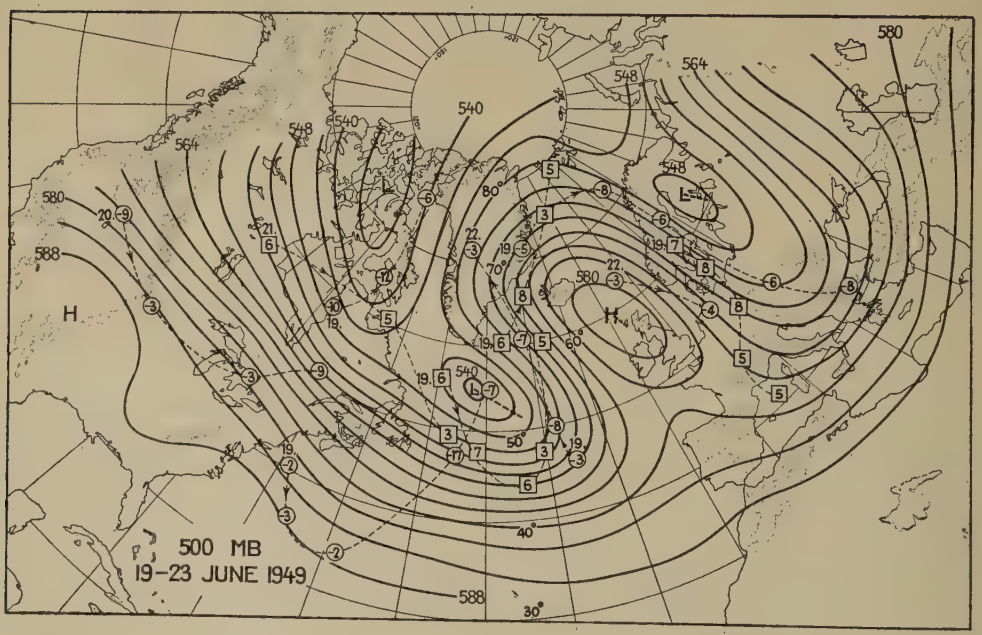
4. The transfer coefficient and the paths of eddies

Eq. (12) has been applied to evaluate the transfer coefficient K, using for $(\partial\bar{z}/\partial t)_e$ the value E from the period 16-29 June (see Fig. 3). The distribution of K is shown in Fig. 4. By far the largest part of the area shows negative K so that eddy transfer was mainly directed against the gradient of mean vorticity. Such transfer leads to local increases in the inequalities of contour height and so to accelerations of the mean flow or else to its maintenance against dissipating effects from turbulence on a much smaller scale than represented by the eddy motion of this study.

The magnitude of K shows considerable variation, but is in most of the area between 10^9 cm^2 sec^{-1} and 10^{10} cm^2 sec^{-1}, i.e., by one to two orders of 10 below Defant's K referred to in the introduction. Since the validity of the diffusion equation ceases in the centre regions of jet-stream patterns (in which the relative vorticity changes sign), the maxima and minima of K lying in these regions have little significance. It has been ascertained, however, that outside the jet regions the inequalities in the distribution of K were considerably less than those of E and $\nabla^2\bar{z}$ in one and the same area.

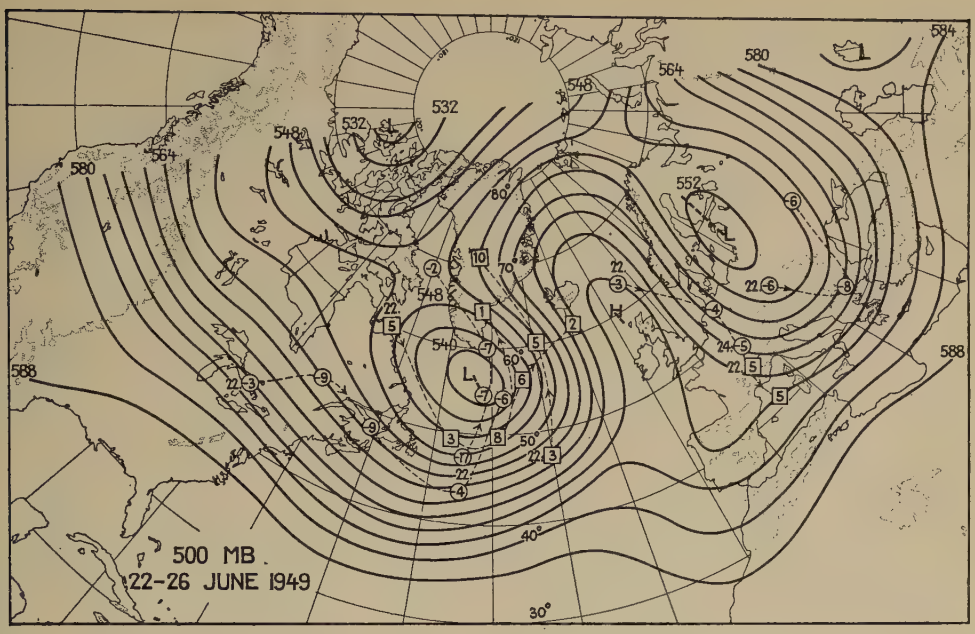


(a)

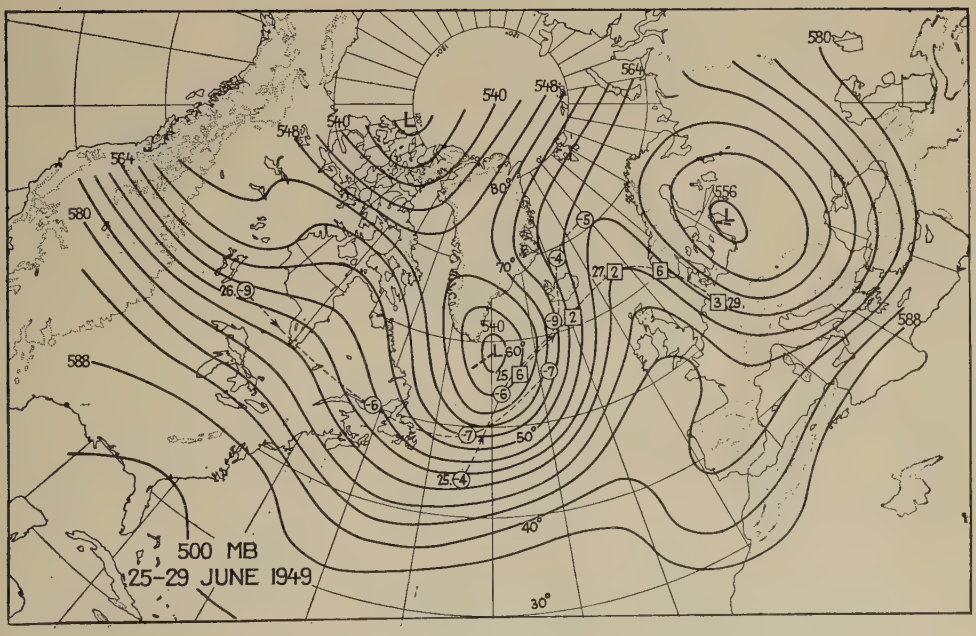


(b)

Figure 5 (a, b). Five-day mean contours of the 500-mb surface. Numbers denote height in decametres. Broken curves are paths of vortex centres in the field of departure from the running five-day mean; circles indicate cyclonic vortices; squares indicate anticyclonic vortices, figures within them the maximum deficit and excess of height respectively; figures beside the symbols denote calendar date.



(c)



(d)

Figure 5 (c, d). Five-day mean contours of the 500-mb surface. Numbers denote heights in decametres. Broken curves are paths of vortex centres in the field of departure from the running five-day mean; circles indicate cyclonic vortices; squares indicate anticyclonic vortices, figures within them the maximum deficit and excess of height respectively; figures beside the symbols denote calendar date.

The average values of K in the large circulation systems over the Atlantic and Europe are shown in Table 4.

The action of the smaller eddies in transferring vorticity from regions with lower to regions with higher vorticity in the mean flow, and thus in maintaining the latter, is directly brought out by the distribution of E relative to the mean flow. It is seen from Fig. 3 that the maxima are found to the right of the jet stream, i.e., in regions of anticyclonic vorticity chiefly associated with anticyclonic lateral shear, the minima to the left in regions of cyclonic vorticity associated with both cyclonic curvature and cyclonic lateral shear.

That the displacement of eddies on the general scale considered here broadly conforms with the assumption of vorticity conservation at the 500-mb level, has been shown by the author (Berson 1951) in a case study of vortex interaction and by Clapp (1952a) in a synoptic-statistical investigation of the integrated effect of hemisphere-wide interaction. Kuo (1951) gave a theoretical treatment of eddy transfer of vorticity on the scale of the general atmospheric circulation, in particular in relation to the meridional displacement of vortices. The criterion for meridional eddy-transfer tendency was derived also by him under the assumption of vorticity conservation, although it was emphasized that vorticity sources associated with baroclinic processes and horizontal divergence were to be regarded as essential in *producing*, in limited areas and at certain times, vorticity concentrations and so vortices.

Thus another way by which to illustrate the mechanism of eddy transfer in the present study is to relate it to the motion of the individual eddies in the mean flow. The daily displacements of the low and high pressure (contour-height) centres in the field of fluctuations z' were evaluated and their tracks obtained by an objective method of smoothing. The tracks of these eddies are shown on the five-day mean maps (Fig. 5). It is observed that the larger part of the cyclonic eddies (minima of z') move across the mean flow, viz., from higher to lower values of $\bar{z}$. This holds especially for cyclonic eddies that were intense or increasing in intensity. The anticyclonic eddies (maxima of z') moved, as a rule, from lower to higher values of $\bar{z}$ but there are more exceptions to this rule than in the case of the cyclones. Fig. 5 shows that to the east of Newfoundland the tracks of both cyclones and anticyclones converged in a rather small region during two weeks, and that they diverged again, having passed that area. It is significant that the region referred to rather closely coincides with the area of largest absolute value of eddy transfer of vorticity, represented by the minimum of E discussed earlier (see Fig. 3). Accordingly the eddy diffusivity is negative throughout this, as one might call it, critical region.

The computations also indicate that in the regions where *anticyclonic* eddies moved from higher to lower values of $\bar{z}$, the eddy diffusivity was found to be positive. Towards the end of the two-week period the blocking high over NW. Europe and the European low show signs of decay. It is seen from Figs. 5 (c), (d) that two eddies of significant intensity during the time of decay moved in a direction opposite to that stated earlier to have been the rule : a cyclonic eddy moved from a point not far from the centre of the Atlantic main low to the northern tip of the high-pressure wedge, marking a four-day increase of $\bar{z}$ (at its centre) by 222 m; an anticyclonic eddy of weak intensity, situated on 27 June near the centre of the wedge, moved subsequently towards the downstream low, its intensity increasing simultaneously though the decrease of $\bar{z}$ at its centre was only 50 m.

In conclusion we may touch upon a process which must be of importance for the evolution, maintenance or decay of large-scale flow during a period of weeks, namely the supply of kinetic energy from heat sources. It is as a rule assumed that vertical

motion, and hence both the vertical transport and divergence term in the vorticity equation, could effect the required changes at a level far removed from the source which is chiefly at the surface of the earth. Clearly, the smaller-scale eddies which we have treated here, derive their kinetic energy from baroclinic developments which *are* accompanied by divergence and vertical transport of vorticity. Once the necessary vorticity concentrations have been created and tropospheric-deep vortices have been formed, they may be taken as masses of air endowed with a definite amount of vorticity : such vortices are driven by the mean flow, tending to preserve their absolute vortex strengths and thus, in accordance with the concept of transfer tendency as suggested by Kuo (1951), to move across the mean (basic) flow in the direction opposite to the gradient of absolute vorticity. It appears, however, from this study that the distinction between absolute and relative vorticity introduces an additional complication in the understanding of the mixing process.

Acknowledgments

The author wishes to express his indebtedness to the Director of the Swedish Meteorological and Hydrological Institute for permission to publish this paper; to Commonwealth Scientific and Industrial Research Organisation, Australia, for generously allowing time for completing it; to Mr. P. F. Clapp and Dr. E. T. Eady, while at the University of Stockholm, and Mr. W. C. Swinbank, Section of Meteorological Physics, C.S.I.R.O., who have read the manuscript and made helpful criticisms.

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The conditions for dynamical similarity of motions of a frictionless perfect-gas atmosphere

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Summary

The object of this paper is to show, in an elementary way, the general significance of the Richardson number for motions of a frictionless perfect gas of variable density subject to gravity. Dimensionless parameters like the Richardson number are important in relation to the concept of dynamical similarity of flow fields, and as a consequence the paper consists largely of discussions of the conditions for dynamical similarity of flow fields of specified kind. It is established that if the flow fields are such that the pressure and density everywhere depart by small fractional amounts only from the values for an equivalent atmosphere in adiabatic equilibrium and if the vertical length scale of the velocity distribution is small compared with the scale height of the atmosphere, the Richardson number is the *sole* parameter governing dynamical similarity. Various possible forms of the Richardson number are examined.

1. Introduction

A few years ago, in a book review published in this *Journal*, I referred to ' the common misconception that the Richardson number has significance as a local quantity,' and I went on to assert ' In fact it is a reference parameter for a whole flow field, just as is the Reynolds number.' Those remarks did not meet with widespread assent, and it has since been urged upon me* that I ought to establish the point in print. This provides part – the lesser part – of the purpose of this note.

At the time I wrote the above review I had thought that there must surely exist somewhere in the literature an examination of the conditions for dynamical similarity of different motions of an atmosphere of variable density and a formal statement of the part played by the Richardson number in ensuring dynamical similarity. I have been unable to find such an analysis in any text-book or journal, and, in view of its absolutely fundamental importance for the presentation and utilization of so many observations in the atmosphere, the major purpose of this note is to remedy this omission. There have of course been theoretical investigations of various particular problems (see, for instance, papers by Taylor (1931) and Schlichting (1935) on the problem of stability of certain laminar flows and the paper by Richardson (1920) on the maintenance of turbulent motion in the presence of a stabilizing density distribution), which have revealed, as an incidental by-product of the analysis, that the Richardson number (or a simple modification of it) is a relevant dimensionless parameter for certain types of flow involving density variations. What I looked for, without success, was a discussion of the general conditions for dynamical similarity, and especially for a statement of the restrictions on the type of flow which are necessary if the Richardson number is to be the *sole* defining parameter.

In short, the purpose of this note is to expound certain generalities about the motion of a fluid of variable density subject to gravity. My belief is that the general theory of dynamical similarity of flow systems with density variations should be an accepted part of the text-book background to meteorological research, and I hope that this paper helps to make it so.

* Principally by K. Calder, whose encouragement I acknowledge with thanks.

2. The concept of dynamical similarity

We say that two fields of flow of a fluid are dynamically similar if the two distributions, with respect to both position and time, of velocity, density, etc., can be made identical by an appropriate choice of the units of length, time and mass in the two cases. A *necessary* condition for it to be possible to make the spatial distributions of velocity identical is clearly that the boundaries have the same geometrical shape in the two cases, and the unit of length to be used will need to be proportional to the linear dimensions of the boundary in each case. Likewise if any temporal variation is imposed on the flows by external agencies, the time scale of this variation will dictate the unit of time to be used in an attempt to make the two velocity distributions identical.

However, geometrical similarity of the boundary shapes will not often be *sufficient* to ensure dynamical similarity. In the case of a uniform viscous incompressible fluid we know* that dynamical similarity requires geometrical similarity of the boundaries *and* equality of the Reynolds numbers of the different flow fields together with identity of the initial and boundary conditions when expressed in non-dimensional form. This dimensionless parameter, the Reynolds number, appears explicitly in the equations of motion when the variables are made non-dimensional (and it is the *sole* parameter appearing therein), or it can be regarded as appearing as the only possible dimensionless combination of the four dimensional parameters known to be relevant to the flow, viz., the length and time (or velocity) scales supplied by the boundary and initial conditions, the density of the fluid, and the viscosity of the fluid.

Thus if a circular cylinder of radius a is suddenly given an angular velocity ω (which remains steady thereafter) about its axis in a viscous incompressible uniform fluid which is initially at rest, we know that the resulting distributions of the non-dimensional velocity $\mathbf{u}/a\omega$ (where $\mathbf{u}$ denotes the vector velocity in the fluid) and pressure $p/\rho a^2\omega^2$ in the fluid will be the same functions of r/a (r = radial position) and $t\omega$ (t = duration of motion) in all such cases, whatever the values of a, ω, ρ (density) and μ (viscosity), provided only that the Reynolds number $R = a^2\omega\rho/\mu$ has the same value. Any field of flow generated by a rotating circular cylinder in this way is specified uniquely by the value of the number $a^2\omega\rho/\mu$. In this sense, the Reynolds number is a reference number for the flow field as a whole.

There is an additional, more direct, physical interpretation of the Reynolds number which can be seen from the way in which it occurs in the equation of momentum. If we define non-dimensional variables :

$$t' = t\omega, \quad \mathbf{x}' = \mathbf{x}/a, \quad \mathbf{u}' = \mathbf{u}/a\omega, \quad p' = p/\rho a^2\omega^2,$$

the vector form of the momentum equation becomes

$$\frac{\partial \mathbf{u}'}{\partial t'} + \mathbf{u}' \cdot \nabla \mathbf{u}' = -\nabla p' + \frac{1}{R}\nabla^2 \mathbf{u}'.$$

Inasmuch as the magnitudes of $\mathbf{u}/a\omega$ and of derivatives of $\mathbf{u}/a\omega$ with respect to $t\omega$ or $\mathbf{x}/a$ are of order unity (for the reason that a and $a\omega$ are representative values of the lengths and velocities occurring in the problem),this equation shows that the ratio of the magnitudes of the inertia and viscous forces acting on unit mass of fluid is of order R. When $R \gg 1$, most aspects of the motion will be determined by inertia forces alone (although, as is well-known, sometimes there are certain singular features of the motion for which viscous forces are important whatever the Reynolds number), and when $R \ll 1$ viscous forces will be dominant. Hence when the Reynolds number is defined in terms of a length

* See Chap. 2 of Prandtl-Tietjens, *Applied Hydro- and Aero-Mechanics* (McGraw-Hill), 1934. This chapter is a good introduction to the general idea of dynamical similarity.

and velocity representative of the boundary shape and of the motion, it is a measure of the order of magnitude of the ratio of inertia to viscous forces for the flow field as a whole.

Sometimes it is possible to subdivide a single field of flow into a number of regions for each of which the boundary conditions on the motion of the enclosed fluid can be simply defined. For instance the laminar boundary layer on a semi-infinite flat plate immersed in a uniform stream can be subdivided in this way. The thickness of the boundary layer increases slowly with distance x from the leading edge of the plate, although the distribution of velocity with respect to distance normal to the plate has approximately the same shape for all x (except near $x = 0$). Hence we can regard the fluid near any point on the plate as having approximately plane parallel motion with a given transverse distribution of velocity, this flow field differing from that near any other point on the plate only inasmuch as the Reynolds number formed from the boundary layer thickness and the free-stream speed is different at the two positions. The boundary layer at position x has its own Reynolds number, and Tollmien and others have shown that this variation of the Reynolds number with x produces, among other effects, a variation of the stability properties of the region of flow in the neighbourhood of position x. This is a sense in which a Reynolds number has some *local* significance ; but note that the localization is only to a certain region of the fluid (and *not* to a point) and is only possible in this case because of the approximate similarity of shape of the velocity distributions in the neighbourhood of different values of x.

It is not possible in general to regard the point-to-point variation of a Reynolds number, however defined, in any given flow field as having any precise meaning. The inertial and viscous forces on unit mass of fluid may each vanish at certain points of the fluid, but this does not necessarily mean that the flows in the neighbourhood of those points are dominated by viscous and inertia forces respectively. In any case, ' inertia ' forces are fictitious and no unambiguous general definition of a Reynolds number at a point in a fluid could be given. In the case of a steady flow through a uniform tube under pressure gradient, none of the fluid elements experiences any acceleration and there are no ' inertia ' forces, because the viscous forces are exactly balanced by the imposed pressure gradient. Nevertheless a Reynolds number based on the tube dimensions and the mean flow (which in turn is related to the pressure gradient) can be used as a reference parameter for the flow as a whole in the manner explained above.

All that has been said concerning the Reynolds number for the motion of a uniform viscous incompressible fluid applies, *mutatis mutandis*, to other dimensionless parameters governing other classes of fluid flow, and in particular to the Richardson number for the flow of an atmosphere of variable density under restrictions to be established below.

3. The class of problems to be considered

The flow fields for which conditions for dynamical similarity will be established herein consist of atmospheric motions on a scale whose upper limit is of the order of the height of the atmosphere and whose lower limit is about, say, 10 cm. The velocities will be assumed to be comparable in magnitude with naturally-occurring winds. On this basis it is permissible to make the following assumptions :

(a) the Coriolis forces can be ignored (since the angular velocities occurring in such motions will be large compared with the earth's angular velocity).

(b) the effects of viscosity and heat conductivity can be ignored (for the reason that the Reynolds number is very large compared with unity).

(c) the Mach number of the fluid is everywhere small.

In addition, in order to avoid too much complexity in the equations, we shall assume that :

(d) the fluid is a perfect gas.

We note from assumption (c) that if appreciable variation in the density of the fluid exists it must be due to the effect of gravity or to an initial inhomogeneity in the fluid. No restrictions will be placed on the density distribution in the first instance. Viscous forces will be ignored but it should be noted that the flow can be turbulent and eddy diffusion processes are not excluded.

4. Conditions for dynamical similarity in the general case

Under the above conditions, the equations representing the balance of momentum and the conservation of mass are

$$\frac{D\mathbf{u}}{Dt} = \mathbf{g} - \frac{1}{\rho}\nabla p \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{1}{\rho}\frac{D\rho}{Dt} = -\nabla . \mathbf{u} \quad . \quad . \quad . \quad . \quad (2)$$

where $\mathbf{u}$, ρ, p represent velocity, density and pressure in the fluid respectively, $\frac{D}{Dt} = \frac{\partial}{\partial t} + \mathbf{u} . \nabla$ is the operator describing rate of change of some quantity associated with a fluid particle as that particle is followed in its motion, and $\mathbf{g}$ denotes the acceleration vector due to gravity. A further equation is supplied by the conservation of energy, which (under the above conditions) expresses the fact that the rate of change of internal energy of an element of fluid equals the rate at which the surrounding fluid does work on the element by compression :

$$\rho c_v \frac{DT}{Dt} = -p \nabla . \mathbf{u} \quad . \quad . \quad . \quad . \quad (3)$$

where T is the absolute temperature of the fluid and c_v (supposed uniform and constant) is the specific heat at constant volume. The system of equations is not yet complete, but is made so by the addition of the perfect-gas law

$$p = (c_p - c_v)\rho T \quad . \quad . \quad . \quad . \quad (4)$$

Temperature plays only an auxiliary role in the problem and can be eliminated from Eqs. (3) and (4) giving

$$\frac{D}{Dt}\left(\frac{p}{\rho^\gamma}\right) = 0, \quad (\gamma = c_p/c_v) \quad . \quad . \quad . \quad (5)$$

which states that changes in the properties of a material element of fluid take place adiabatically. Eqs. (1), (2) and (5) will be regarded as the governing equations of the motion.

In order to make the variables non-dimensional we introduce the following representative quantities :

$$L, U, \rho_0, p_0,$$

which we assume to be supplied by the data of the problem (i.e., by the spatial and temporal boundary conditions). The most convenient choice of these quantities* will be a matter for consideration in each particular flow problem.

* The general procedure should be to introduce one representative quantity for each of the variables in the equations, but sometimes it is at once evident that a smaller number can be chosen, with a consequent reduction in the number of dimensionless parameters that can be formed from these representative quantities. For instance, if a representative length L and a representative velocity U are introduced, it is immediately clear that one of the conditions for dynamical similarity of two flow fields will be that their representative time intervals are the same multiple of L/U in the two cases. Thus we can use L/U as the representative time interval and thereby avoid introducing a representative interval τ which, so we should find, has to be such that $\tau U/L$ has the same value for dynamically similar flow fields. It will be asked why the representative pressure p_0 is introduced, when $\rho_0 U^2$ has the appropriate dimensions and could serve as a unit of pressure. The reason is that $\rho_0 U^2$ is an inconveniently small unit of absolute pressure, as explained later, and it is worth while here to allow an extra dimensionless parameter to appear in the analysis

In cases in which there is an appreciable difference between the length scales of the horizontal and vertical distributions of $\mathbf{u}$ and ρ, L should be chosen to be representative of the distributions in the vertical since it is with the effect of gravity that we are concerned primarily. We now define dimensionless variables

$$\mathbf{u}' = \mathbf{u}/U, \quad \rho' = \rho/\rho_0, \quad p' = p/p_0, \quad \mathbf{x}' = \mathbf{x}/L, \quad t' = tU/L,$$

and rewrite the set of governing equations as

$$\frac{D\mathbf{u}'}{Dt} = \frac{\mathbf{g}L}{U^2} - \frac{\nabla p'}{\rho'}\frac{p_0}{\rho_0 U^2} \quad . \quad . \quad . \quad (6)$$

$$\frac{1}{\rho'}\frac{D\rho'}{Dt'} = -\nabla . \mathbf{u}' \quad . \quad . \quad . \quad . \quad (7)$$

$$\frac{D}{Dt'}\left(\frac{p'}{\rho'^{\gamma}}\right) = 0 \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where ∇ is now a differential operator with respect to $\mathbf{x}'$.

Now any two flows coming within the category under consideration will be dynamically similar, if, and only if, they are described by identical solutions of the set of Eqs. (6), (7) and (8), for it would not otherwise be possible to bring the two velocity distributions into coincidence by an appropriate change of scales. Two solutions of the above set of equations will be identical if, and only if, the numerical coefficients in the equations have the same values in the two cases and the boundary conditions (with respect to both independent variables $\mathbf{x}'$ and t') which the dependent variables must satisfy are the same in the two cases. There are three numerical coefficients occurring in the equations :

(i) $\mathbf{g}\,L/U^2$, which is (the reciprocal of) a Froude number and plays a specially important part in the problems in which a free surface appears.

(ii) $p_0/\rho_0 U^2$, which is equal to $1/\gamma M_0^2$ (M_0 = Mach number corresponding to velocity U and the velocity of sound under conditions described by ρ_0 and p_0). $p_0/\rho_0 U^2$ is a very large number for the ' low-speed ' problems under consideration and it is more convenient to work with the combination $\dfrac{p_0}{\rho_0 U^2}\Big/\dfrac{gL}{U^2} = \dfrac{p_0}{\rho_0 gL}$ (there being no need to specify the direction of gravity a second time) which is the ratio of the height of a homogeneous atmosphere of density ρ_0 and pressure p_0 at the base to the length L.

(iii) γ, which is a property of the medium.

Regarding the boundary conditions, we do not know enough about the differential equations concerned to be able to say with certainty what conditions must be specified to make the problem determinate, but it is a plausible inference from physical experience that $\mathbf{u}'$ and ρ' must be given as functions of t' at all spatial boundaries and $\mathbf{u}'$, ρ', p' must be given as functions of $\mathbf{x}'$ at an initial value of t' ; it seems certain that such a set of boundary conditions is sufficient, although in some circumstances the conditions may well be over-sufficient.

We can thus formulate the conditions for dynamical similarity of two flow fields which come within the class described in section 3 as follows :

(a) the values of $\mathbf{g}L/U^2$, $p_0/\rho_0\, gL$, and γ must be the same in the two cases;

(b) The spatial boundaries must be geometrically similar, and $\mathbf{u}'$ and ρ' must be the same given functions of t' at the values of $\mathbf{x}'$ defined by these boundaries, in the two cases;

(c) $\mathbf{u}'$, ρ' and p' must be the same given functions of $\mathbf{x}'$ at the initial value of t' in the two cases.

These conditions may take simpler forms in special cases. For instance if the two flow fields concerned are *steady*, no initial conditions on $\mathbf{u}'$, ρ' and p' are needed (provided

the spatial boundaries are such that every streamline crosses them at least once). Again, if it should happen, as is the case in problems of free convection, that no representative velocity U is supplied by the data of the problem, the parameter $\mathbf{g}L/U^2$ drops out of the condition (a).

We note that no parameter resembling the Richardson number has made its appearance. Evidently the class of problems for which the Richardson number is the governing parameter must be – if it exists at all – more restricted than the class of problems considered above. We proceed to consider various restrictions on the class of flow fields in order to bring the Richardson number into the picture.

5. Flow fields in which the pressure is everywhere close to the value for adiabatic equilibrium

In the class of problems just considered the pressure distribution is not restricted in any way, and although this generality may occasionally be useful a common case will be that in which the distribution of absolute pressure is approximately as for an atmosphere in adiabatic equilibrium. Let p_a, ρ_a, T_a be quantities appropriate to an atmosphere which has the same total mass and the same total internal energy (and the same value of γ) as the actual atmosphere under consideration (this will be called an 'equivalent' atmosphere), and which is in adiabatic equilibrium; then if z is the vertical coordinate, we have

$$\left(\frac{p_a}{p_{a0}}\right)^{\frac{\gamma-1}{\gamma}} = \left(\frac{\rho_a}{\rho_{a0}}\right)^{\gamma-1} = \frac{T_a}{T_{a0}} = 1 - \frac{(\gamma-1)\, g\rho_{a0}}{\gamma p_{a0}}(z - z_0) \quad . \quad . \quad (9)$$

where p_{a0}, ρ_{a0}, T_{a0} are the values of p_a, ρ_a, T_a at a reference plane $z = z_0$ and are determined by the total mass and total internal energy of the actual atmosphere. The assumption about the flow fields is that they are all such that

$$\frac{p - p_a}{p_a} \ll 1 \quad . \quad . \quad . \quad . \quad (10)$$

There is no imposed restriction, for the moment, on the magnitude of $\frac{\rho - \rho_a}{\rho_a}$ (nor, consequently, on $\frac{T - T_a}{T_a}$). The kind of problem within this class is one in which the density variations may be large but large deviations (with the same sign) from the value ρ_a do not persist over large vertical distances.

Under these conditions, p/p_a is everywhere close to unity and Eq. (8) therefore reduces to the statement that ρ/ρ_a is approximately constant for a moving particle of fluid. Consequently

$$\frac{1}{\rho}\frac{D\rho}{Dt} \simeq \frac{1}{\rho_a}\frac{D\rho_a}{Dt},$$

$$= \frac{1}{\rho_a} w \frac{d\rho_a}{dz} = -\frac{g\rho_a}{\gamma p_a} w, \quad . \quad . \quad . \quad (11)$$

in view of Eq. (9), where w is the vertical component of $\mathbf{u}$. The set of governing equations becomes :

$$\frac{D\mathbf{u}'}{Dt'} = \frac{\mathbf{g}L}{U^2} - \frac{\nabla p'}{\rho'}\frac{p_0}{\rho_0 U^2} \quad . \quad . \quad . \quad (12)$$

$$\nabla \cdot \mathbf{u}' = w' \frac{\rho_a'}{\gamma p_a'}\frac{\rho_0 g L}{p_0} \quad . \quad . \quad . \quad (13)$$

$$\frac{D}{Dt'}\left(\frac{\rho'}{\rho_a'}\right) = 0 \quad . \quad . \quad . \quad . \quad . \quad (14)$$

where $\rho_a' = \rho_a/\rho_0$, $p_a' = p_a/p_0$. The conditions for dynamical similarity of two flow fields satisfying the required assumptions are then as in Section 4, except that the spatial distribution of p/p_0 at an initial instant is now given approximately as part of the data of the problem and need no longer be mentioned in the conditions for similarity.

This particular restriction, viz., that $\frac{p - p_a}{p_a} \ll 1$, has not yielded the Richardson number as a governing parameter but we shall be able to see what further restrictions are needed to this end if we make a slight change of the variables in the Eqs. (12), (13), (14). If we use the pressure difference $(p - p_a)$ and the density difference $(\rho - \rho_a)$ as new variables, and recall that $\nabla p_a = \rho_a \mathbf{g}$, the equations become, in the dimensional form,

$$\left(\frac{\rho - \rho_a}{\rho_a} + 1\right)\frac{D\mathbf{u}}{Dt} = \mathbf{g}\left(\frac{\rho - \rho_a}{\rho_a}\right) - \frac{\nabla(p - p_a)}{\rho_a} \quad . \quad . \quad (15)$$

$$\nabla \cdot \mathbf{u} = w\frac{g\rho_a}{\gamma p_a} \quad . \quad . \quad . \quad . \quad . \quad (16)$$

$$\frac{D}{Dt}\left(\frac{\rho - \rho_a}{\rho_a}\right) = 0 \quad . \quad . \quad . \quad . \quad . \quad (17)$$

The boundary conditions for the problem need now be given only in terms of density differences, but as compensation for this less comprehensive specification of the problem more dimensionless parameters appear in the equations. If $\delta\rho_0$ is a representative value of $(\rho - \rho_a)$ which is supplied by the data of the problem and which can be used to make the variable $(\rho - \rho_a)$ dimensionless, the conditions for dynamical similarity of two flow fields are seen to be :

(a) the values of $\mathbf{g}L/U^2$, $p_0/\rho_0 gL$, γ and $\delta\rho_0/\rho_0$ must be the same in the two cases;

(b) the spatial boundaries must be geometrically similar, and $\mathbf{u}'$ and $\frac{\rho - \rho_a}{\delta\rho_0}$ must be the same given functions of t' on these boundaries, in the two cases;

(c) $\mathbf{u}'$ and $\frac{\rho - \rho_a}{\delta\rho_0}$ must be the same given functions of $\mathbf{x}'$ at the initial value of t' in the two cases.

If it were not for the occurrence of the variable $\frac{\rho - \rho_a}{\rho_a}$ on the left-hand side of Eq. (15) the dimensionless parameters $\delta\rho_0/\rho_0$ and $\mathbf{g}L/U^2$ would arise only in (multiplicative) combination, and we recognize this combination as a modified form of what has hitherto been termed the Richardson number. The next restriction therefore is that $\frac{\rho - \rho_a}{\rho_a}$ is small enough for it to be negligible on the left-hand side of Eq. (15).

6. Flow fields such that $\frac{p - p_a}{p_a} \ll 1$ and $\frac{\rho - \rho_a}{\rho_a} \ll 1$

The assumption here is that both the pressure and the density depart by small (relative) amounts only from the values for an equivalent atmosphere in adiabatic equilibrium. This does not mean of course that $\frac{p - p_a}{p_a}$ and $\frac{\rho - \rho_a}{\rho_a}$ are neglected wherever they occur; they are neglected where they occur as perturbation terms, but where, as in the two terms on the right-hand side of Eq. (15), they occur as first-order effects they are retained. Eq. (15) thus becomes

$$\frac{D\mathbf{u}}{Dt} = \mathbf{g}\left(\frac{\rho - \rho_a}{\rho_a}\right) - \frac{\nabla(p - p_a)}{\rho_a} \quad . \quad . \quad . \quad (18)$$

Equation (16) is unchanged, but Eq. (17) must be modified to allow for the fact that $\frac{\rho - \rho_a}{\rho_a}$ is now in general as small as $\frac{p - p_a}{p_a}$. On returning to Eq. (5), from which Eq. (17) was derived, and putting

$$\frac{p}{\rho^\gamma} = \frac{p_a}{\rho_a{}^\gamma} \frac{1 + \frac{p - p_a}{p_a}}{\left(1 + \frac{\rho - \rho_a}{\rho_a}\right)^\gamma} \simeq \frac{p_a}{\rho_a{}^\gamma}\left[1 + \frac{p - p_a}{p_a} - \gamma\left(\frac{\rho - \rho_a}{\rho_a}\right)\right]$$

we find that the equation appropriate to the present assumptions is

$$\frac{D}{Dt}\left[\left(\frac{\rho - \rho_a}{\rho_a}\right) - \frac{1}{\gamma}\left(\frac{p - p_a}{p_a}\right)\right] = 0 \quad . \quad . \quad . \quad (19)$$

The quantity in square brackets in Eq. (19) vanishes for an element which can be brought adiabatically to the state (ρ_a, p_a) from its current state (ρ, p), and an immediate suggestion is that it is related to the potential density; we might in any case have expected the potential density $\overset{*}{\rho}$ to be a useful concept here in view of the fact that the pressure plays a secondary role in the problem. We define $\overset{*}{\rho}$ and $\overset{*}{T}$ as the density and temperature of an element of fluid when it is brought adiabatically to some reference pressure which we shall take as p_0. Hence

$$\overset{*}{\rho} = \rho\left(\frac{p_0}{p}\right)^{\frac{1}{\gamma}}, \quad \overset{*}{T} = T\left(\frac{p_0}{p}\right)^{\frac{\gamma-1}{\gamma}} \quad . \quad . \quad . \quad (20)$$

and if we choose the representative density ρ_0 to be equal to the (constant) potential density $\overset{*}{\rho}_a$ of the equivalent atmosphere in adiabatic equilibrium (which of course puts restrictions on the choice of the representative pressure p_0), we find

$$\frac{\overset{*}{\rho} - \overset{*}{\rho}_a}{\overset{*}{\rho}_a} = \frac{\rho}{\rho_a}\left(\frac{p_a}{p}\right)^{\frac{1}{\gamma}} - 1,$$

$$\simeq \left(\frac{\rho - \rho_a}{\rho_a}\right) - \frac{1}{\gamma}\left(\frac{p - p_a}{p_a}\right) \quad . \quad . \quad . \quad (21)$$

provided $\frac{\rho - \rho_a}{\rho_a}$ and $\frac{p - p_a}{p_a}$ are of equal order of smallness.

The governing equations, (18), (16) and (19), can now be written, with the aid of Eq. (9), and without further approximation beyond that contained in (21), as

$$\frac{D\mathbf{u}}{Dt} = \mathbf{g}\left(\frac{\overset{*}{\rho} - \overset{*}{\rho}_a}{\overset{*}{\rho}_a}\right) - \nabla\left(\frac{p - p_a}{\rho_a}\right) \quad . \quad . \quad (22)$$

$$\nabla \cdot \mathbf{u} = w\frac{g\rho_a}{\gamma p_a} \quad . \quad . \quad . \quad . \quad . \quad (23)$$

$$\frac{D}{Dt}\left(\frac{\overset{*}{\rho} - \overset{*}{\rho}_a}{\overset{*}{\rho}_a}\right) = 0 \quad . \quad . \quad . \quad . \quad . \quad (24)$$

and the conditions for dynamical similarity of two flow fields which conform to these equations are :

(a) the values of $\frac{gL}{U^2}\frac{\delta\overset{*}{\rho}_0}{\overset{*}{\rho}_a}$, $\frac{p_0}{\rho_0 gL}$ and γ must be the same in the two cases;

(b) the spatial boundaries must be geometrically similar, and $\mathbf{u}'$ and $\frac{\overset{*}{\rho} - \overset{*}{\rho}_a}{\delta\overset{*}{\rho}_0}$ must be the same given functions of t' on these boundaries, in the two cases;

(c) $\mathbf{u}'$ and $\frac{\overset{*}{\rho} - \overset{*}{\rho}_a}{\delta\overset{*}{\rho}_0}$ must be the same given functions of $\mathbf{x}'$ at the initial value of t' in the two cases. (In these conditions $\delta\overset{*}{\rho}_0$ is a representative value of $\overset{*}{\rho} - \overset{*}{\rho}_a$ which is supplied by the data of the problem.)

The appearance of $\frac{gL\,\delta\overset{*}{\rho}_0}{U^2\overset{*}{\rho}_a}$ (the relation between this parameter and the Richardson number as conventionally defined will be considered later) as one of the governing parameters for a certain class of flows with density variations achieves one of the objects of this analysis, although we have yet to find a class of motions for which it is the sole governing parameter. The set of Eqs. (22), (23) and (24) has some further interesting features. The density ρ_a has effectively disappeared from Eq. (22), since it can be regarded as part of a new dependent variable $\frac{p - p_a}{\rho_a}$ (with dimensions [velocity]^2) whose distribution will be determined as part of the solution of the problem. The variation of ρ_a and p_a with height is relevant to the term on the right-hand side of Eq. (23) only. Hence this latter term alone is responsible for the appearance of both $p_0/\rho_0 gL$ and γ in the group of governing parameters, and we have a clue, which will be taken up in the following section, to the further restriction necessary to make the Richardson number the sole governing parameter.

A special case which is covered by Eqs. (22), (23) and (24) and which may have some practical value in meteorology may be noticed here in passing. For a fluid which is such that $\frac{\overset{*}{\rho} - \overset{*}{\rho}_a}{\overset{*}{\rho}_a} = 0$ at all points (i.e., the potential density is uniform) at the initial instant' the equations take the very simple form

$$\frac{D\mathbf{u}}{Dt} = -\nabla\left(\frac{p - p_a}{\rho_a}\right) \quad . \quad . \quad . \quad . \quad (25)$$

$$\nabla \cdot \mathbf{u} = w\frac{g\rho_a}{\gamma p_a} \quad . \quad . \quad . \quad . \quad . \quad (26)$$

the third equation stating simply that the potential density continues to be the same at all points. If $\boldsymbol{\omega} = \nabla \times \mathbf{u}$ is the vector vorticity, Eq. (25) shows that

$$\frac{D\boldsymbol{\omega}}{Dt} - \boldsymbol{\omega} \cdot \nabla\mathbf{u} = 0\,;$$

hence if the vorticity of an element of fluid is zero initially it will remain zero as the particle moves. If the vorticity is zero *everywhere* initially it will remain so, and if a velocity potential ϕ such that $\mathbf{u} = \nabla\phi$ exists at the initial instant it will continue to exist. Eq. (26) then shows that such potential flows must conform to

$$\nabla^2\phi = \frac{g\rho_a}{\gamma p_a}\frac{\partial\phi}{\partial z} \quad . \quad . \quad . \quad . \quad . \quad (27)$$

where $g\rho_a/\gamma p_a$ is the reciprocal of a linear function of z (see Eq. (9)). This equation, which could certainly be solved numerically in simple cases, describes the irrotational flow of a fluid of uniform potential density which is such that $\frac{p - p_a}{p_a}$ remains small everywhere. For instance it would apply to the flow of an atmosphere over a mountain range, provided the fluid far upstream of the range is in adiabatic equilibrium and has a uniform horizontal velocity. It seems possible that solutions describing the two-dimensional flow over simple shapes such as half an elliptic cylinder resting on a plane might provide an approximate representation of some of the flow fields encountered in the earth's atmosphere.

7. Flow fields such that $\frac{p - p_a}{p_a} \ll 1$, $\frac{\rho - \rho_a}{\rho_a} \ll 1$, and the vertical scale of the motion is small

Finally, we introduce a restriction which will have the effect of removing $p_0/\rho_0 gL$ and γ from the list of governing parameters. As already remarked, these dimensionless parameters arise, under the conditions considered in the preceding section, from the presence of the term on the right-hand side of Eq. (23). Now $p_a/g\rho_a$ is the thickness of the layer which all that part of the atmosphere above the position under consideration would occupy if its density were made uniform and equal to ρ_a; in other words it is a 'scale height,' which is a measure of the vertical distance over which density variations are appreciable. We now make the assumption that the flow fields under discussion are such that the distance over which appreciable changes in the velocity occur are small compared with the length $p_a/g\rho_a$, so that the order of magnitude of $\nabla \cdot \mathbf{u}$ will in general be large compared with that of $w\frac{g\rho_a}{\gamma p_a}$. But according to Eq. (23) their numerical values are the same. Hence the numerical value of $\nabla \cdot \mathbf{u}$ must be much less than its general order of magnitude, and the effective form of Eq. (23) is

$$\nabla \cdot \mathbf{u} \simeq 0 \qquad . \qquad . \qquad . \qquad . \qquad (28)$$

An equivalent statement of the assumption is that the density is approximately constant over distances which are representative of the distribution of velocity. In fact, as Eq. (28) shows, we are assuming conditions under which the fluid behaves as though it were *incompressible*.

(If we return to the general Eqs. (6), (7) and (8), and do not make the assumptions that $\frac{p - p_a}{p_a}$ and $\frac{\rho - \rho_a}{\rho_a}$ are small, it is still possible to show that the fluid behaves as though it were incompressible provided the length scale of the velocity distribution is small compared with the scale height $p/g\rho$ and provided the time scale defined by the value of $\nabla \cdot \mathbf{u}$ is small compared with the time required for an appreciable change in pressure to occur. These conditions will normally be satisfied by motions on a length scale less than, say, about 100 m occurring in the earth's atmosphere.)

Thus the equations describing the motion in this case are (22), (24) and (28), and the parameters $p_0/\rho_0 gL$ and γ no longer appear in the equations when the variables are made dimensionless. The conditions for dynamical similarity of two flow fields are as in the preceding section, except that condition (a) reduces to :

(a) the value of $\frac{gL\,\delta\rho_0^*}{U^2\rho_a^*}$ must be the same in the two cases.

We have thus achieved the object of the analysis, viz., to establish the class of flow systems for which the Richardson number is the sole governing parameter. This class of flow systems has been found to consist of low-speed motions of a frictionless atmosphere such that the pressure and density everywhere depart by small fractional amounts only from the values for an equivalent atmosphere in adiabatic equilibrium and such that the vertical length scale of the velocity distribution is small compared with the scale height of the atmosphere.

The empirical conclusion of Deacon (1949) and Pasquill (1949) that the Richardson number is a relevant parameter for the specification of turbulent flow in the lowest few metres of the atmosphere under non-adiabatic conditions thus has a sound analytical basis. As the above discussion shows, we can go further and assert that, apart from the effect of molecular diffusion and apart from differences in the boundary conditions of different flow fields, equality of the Richardson numbers is a *sufficient* condition for flow fields of this kind to be dynamically similar.

8. Alternative definitions of the Richardson number

It remains now to point out that, inasmuch as the choice of the representative quantities L, U and $\delta\overset{*}{\rho}_0$ is not unique, several alternative forms of the parameter $\dfrac{gL\,\delta\overset{*}{\rho}_0}{U^2\overset{*}{\rho}_a}$ may be used. A similar latitude in the definition of a Reynolds number exists. For laminar flow through a pipe under pressure gradient the representative velocity could be chosen as the mean velocity across the pipe section, or as the velocity at the centre of the pipe, or even as the product of the pipe diameter and the velocity gradient at the wall; the most convenient choice would depend on the data supplied by the problem (and if only the pressure gradient is given, none of the above velocities would be a convenient choice).

Now in many flow problems it happens that there is no clearly defined length. For instance in boundary-layer problems the thickness of the boundary-layer is a representative length for the distribution of velocity, but this thickness can be defined precisely only by means of an integral (e.g., the displacement and momentum thicknesses) or by means of some arbitrary definition of the 'edge' of the boundary layer (e.g., as the position where the velocity is 95 per cent of its asymptotic value). This difficulty about the choice of a representative length arises often in problems of the flow in the earth's friction layer. A convenient way around the difficulty is to replace U/L by a representative velocity gradient and $\delta\overset{*}{\rho}_0/L$ by a representative density gradient. If $\left(\dfrac{\partial u}{\partial z}\right)_0$ and $\left(\dfrac{\partial \overset{*}{\rho}}{\partial z}\right)_0$ are velocity and (potential) density gradients representative of the vertical distributions of velocity and density, a choice of governing parameter which is entirely equivalent to $\dfrac{gL\,\delta\overset{*}{\rho}_0}{U^2\overset{*}{\rho}_a}$ for all the analysis described in this paper is

$$\frac{g\left(\dfrac{\partial \overset{*}{\rho}}{\partial z}\right)_0}{\overset{*}{\rho}_a\left(\dfrac{\partial u}{\partial z}\right)_0^2} \qquad . \quad . \quad . \quad . \quad . \quad (29)$$

$\left(\dfrac{\partial u}{\partial z}\right)_0$ and $\left(\dfrac{\partial \overset{*}{\rho}}{\partial z}\right)_0$ might well be the velocity and density gradients at a solid boundary (this was the choice made by Schlichting (1935) in his discussion of stability of laminar flow), although the values at a standard (dimensionless) height might be a more convenient choice in turbulent flow over a rough boundary. The expression (29) (perhaps with the substitution of some other representative density for $\overset{*}{\rho}_a$ and with the substitution of temperatures for densities) seems to have a fixed place in the literature as *the* Richardson number, but there is no reason why, as with the Reynolds number, several different forms of the dimensionless parameter should not have the same name; the definition of the Richardson number to be used in a particular problem should in any case be stated clearly.

For many experimental purposes the temperature is a more convenient variable than the density. When $\dfrac{p-p_a}{p_a}$ and $\dfrac{\rho-\rho_a}{\rho_a}$ are both small compared with unity, we have, with the aid of Eq. (20),

$$\frac{\overset{*}{T}-\overset{*}{T}_a}{\overset{*}{T}_a} \simeq \left(\frac{T-T_a}{T_a}\right) - \frac{\gamma-1}{\gamma}\left(\frac{p-p_a}{p_a}\right) \qquad . \quad . \quad (30)$$

$$\simeq -\frac{\overset{*}{\rho}-\overset{*}{\rho}_a}{\overset{*}{\rho}_a} \qquad . \quad . \quad . \quad . \quad . \quad (31)$$

Thus Eqs. (22) and (24) can be written with $-\left(\frac{\overset{*}{T}-\overset{*}{T}_a}{\overset{*}{T}_a}\right)$ in place of $\left(\frac{\overset{*}{\rho}-\overset{*}{\rho}_a}{\overset{*}{\rho}_a}\right)$, and the Richardson number in the form (29) can be written equally (ignoring the minus sign here) as

$$\frac{\mathbf{g}\left(\frac{\partial \overset{*}{T}}{\partial z}\right)_0}{\overset{*}{T}_a\left(\frac{\partial u}{\partial z}\right)_0^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

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Transverse flow at entrance and exit to jet streams

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Summary

From a statistical examination of wind observations near jet streams, two conclusions are reached :

(a) In the jet-stream entrance region there is evidence of a component of wind towards the left (looking downstream), and this reaches a maximum at the level of the jet centre and falls off above and below. It is probable that this arises from an ageostrophic flow across the contours as a consequence of the acceleration of the air downstream. The transverse flow would be consistent with the existence of a direct thermal circulation either above or below the jet stream, or both.

(b) In the exit region the cross-stream wind component is towards the right (looking downstream), and it reaches a maximum at the level of the jet centre and falls off above and below. This probably arises from an ageostrophic motion across the contours as a result of the deceleration of the air downstream. It would be consistent with an indirect thermal circulation either above or below the jet stream, or both.

For a sample of observations over the British Isles, the jet axis in entrance regions is in the mean about 2,000 ft (27 mb) higher than in exit regions.

1. Introduction

Theory indicates that an ageostrophic flow across the isobars occurs in regions of confluence and difluence in the wind field (e.g., Sutcliffe 1938) : looking downwind, this cross-flow should be to the left in the confluence and to the right in the difluence. The effect should be particularly pronounced in the entrance and exit regions of jet streams. Indeed Namias and Clapp (1949) specifically refer to the cross-isobar flow in the region of confluence as an essential element in the jet-stream mechanism. Sawyer (1951) invoked the existence of cross-flow in the confluence region to explain the formation of a ' folded ' tropopause. It was therefore considered desirable to examine wind observations in the neighbourhood of jet streams to see whether the cross-jet wind components could be detected and, if so, how these components varied immediately below and above the jet axis.

2. Method

By inspection of the 15 GMT 300-mb contour charts the positions of jet streams in the vicinity of the British Isles were estimated. For this purpose a jet stream was regarded as defined by more or less straight and parallel contours with maximum wind speed greater than 70 kt and with the contour gradient decreasing on both sides of the maximum. Wind observations within about 200 mi of the axis of such jet streams were extracted. All observations were placed into one of six groups, namely, observations near the right and left entrance, the right and left centre and the right and left exit of the jet stream. The entrance was taken as the region where the 300-mb contours showed noticeable confluence, the exit the region of noticeable difluence. Observations in entrance or exit regions which were made more than about 200 mi from the zone of sensibly parallel contours were not used. The six sectors are shown schematically in Fig. 1.

The level of the jet stream was taken as the level of the maximum wind reported on each sounding (winds were reported only at 50-mb levels). The axis of the jet stream was defined by the approximately straight and parallel contours on the 300-mb chart in the region of the jet. The wind components perpendicular to this axis were evaluated on each sounding at 50-mb intervals below and above the level of the jet. The wind component was regarded as positive for cross-flow from left to right, looking downstream, and as negative for flow from right to left.

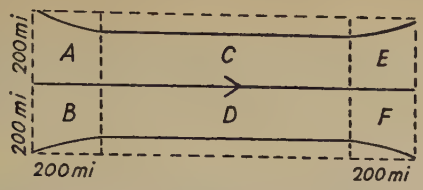

Figure 1. Sectors of the jet stream used in classification of wind observations. A = left entrance, B = right entrance, C = left centre, D = right centre, E = left exit, F = right exit. Schematic contours at 300 mb as continuous lines.

The method of analysis has obvious shortcomings. In the first place, the jet axis at 300 mb cannot be placed accurately, although it is most unlikely to be more than 200 mi from the estimated position; on the other hand the direction of the jet is probably little different from the estimated direction. Secondly, the height of the jet axis need not necessarily be the same as the height of the wind maximum observed at a point near the jet, but the difference is generally small – normally this procedure underestimates the height of the axis when using soundings to the left and overestimates it with those to the right of the jet stream. Thirdly, the classification of observations into the six categories by visual inspection of the 300-mb charts must be subjective to some extent. However, a statistical analysis of the data should enable the main features to emerge, even though they may be obscure or uncertain in any particular case.

3. The mean cross-flow

The components of the wind perpendicular to the jet axis were calculated from each sounding, and the mean values of the transverse components in the various sectors are presented in Table 1. Only those wind reports which reached at least 100 mb above the level of the jet centre were used.

TABLE 1. Mean cross-jet flow (kt) at levels above and below jet stream axis. Positive values indicate flow to right, looking downstream

Region		Above jet 100 mb	Above jet 50 mb	At jet 0	Below jet 50 mb	Below jet 100 mb	Below jet 150 mb	Below jet 200 mb	No. of obs.
Entrance	left	− 3·4	− 2·8	− 6·2	− 3·7	− 1·0	2·4	3·2	22
	right	− 6·2	− 6·5	− 10·1	− 9·2	− 6·9	− 4·9	− 2·0	21
	combined	− 4·8	− 4·6	− 8·1	− 6·4	− 3·8	− 1·1	0·7	43
Centre	left	1·0	1·0	1·9	4·8	3·2	2·4	2·3	30
	right	2·5	5·2	5·9	5·0	4·7	5·1	3·7	24
	combined	1·7	2·9	3·7	4·9	3·8	3·6	2·9	54
Exit	left	4·5	5·5	7·3	3·1	2·1	− 2·4	− 4·2	29
	right	5·9	10·2	12·0	5·6	3·1	1·7	− 0·9	30
	combined	5·2	7·9	9·7	4·4	2·6	− 0·4	− 2·5	59

In the initial stages of the work observations for the year 1949 only were used. Although giving results broadly similar to those in Table 1, the number of wind soundings in the entrance and exit regions were insufficient to be conclusive. The data were therefore extended to include data from other years to bring the number of soundings in each sector to more than 20. Thus the values for the entrance region are based on the three-year period 1948-50; those for the central region are based on 1949 only, and those for the exit on the years 1949-50.

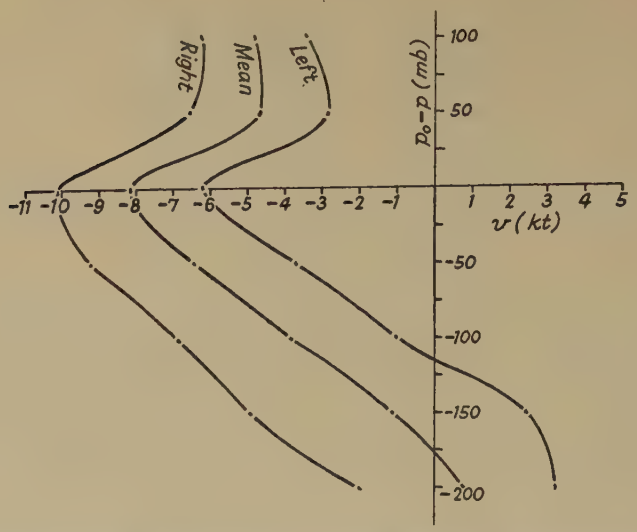

Figure 2. Mean transverse wind components in jet-stream entrance. The abscissa is the mean cross-jet wind component (v) in kt, positive for flow to right looking downstream. The ordinate is the mean vertical pressure difference ($p_0 - p$) in mb, where p_0 is the pressure at the level of the jet axis; ($p_0 - p$) is positive for levels above the jet axis.

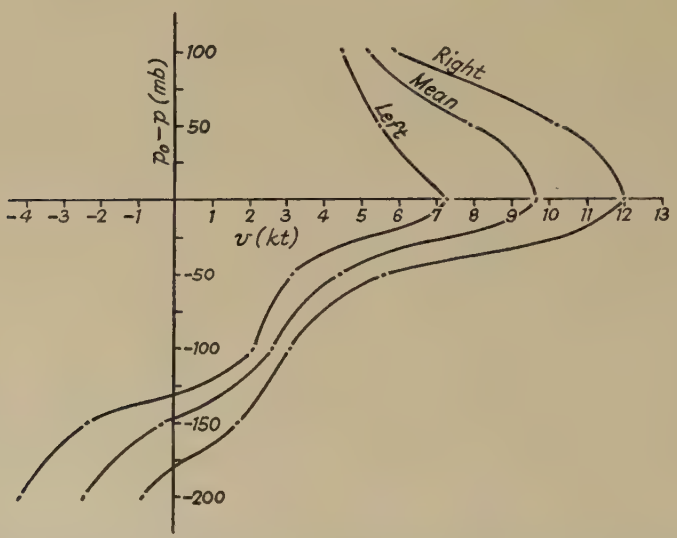

Figure 3. Mean transverse wind components in jet-stream exit. The abscissa is the mean cross-jet wind component (v) in kt, positive for flow to right looking downstream. The ordinate is the mean vertical pressure difference ($p_0 - p$) in mb, where p_0 is the pressure at the level of the jet axis; ($p_0 - p$) is positive for levels above the jet axis.

The mean components in the entrance and exit regions are represented graphically in Figs. 2 and 3 respectively.

Fig. 2 shows that the maximum cross-flow in the entrance region is at the level of the jet axis, and is from right to left, looking downstream. The profiles for the right, left and combined sectors are essentially similar. The magnitudes of the mean wind components in the right and left sectors differ by some 4 kt at each level, but this effect is probably due to the difference in direction of the contour lines (and geostrophic wind) between the right and the left sectors. Observations in the right sector must usually

have a negative geostrophic wind component perpendicular to the jet axis; whereas in the left sector the streamlines are such that a positive geostrophic wind component must normally occur. However, combining the right and left sector observations produces the mean profile of Fig. 2, which should represent the cross-circulation at the jet axis fairly accurately. The cross-components are probably ageostrophic winds across the contours resulting from the acceleration of the air downstream.

Supporting evidence for the reality of the mean profile of Fig. 2 is given by examination of the statistical data employed. The mean of the difference between the cross-components at the level of the jet axis and 200 mb below is − 8·8 kt and the standard error of our estimate of the mean is 2·1 kt (observations in right and left sectors combined); this mean difference is highly significant, according to Student's ' t ' test. Also, the mean of the difference between the cross-components at the jet level and 100 mb above is − 3·36 kt and the standard error of our estimate of the mean is 1·7 kt; this mean is nearly significant at the 5 per cent level of significance. Thus it is almost certain that the difference between the transverse components at the jet level and 200 mb below is real, whereas there is a 5 per cent probability that the difference between the components at the jet level and 100 mb above is a chance one. In other words the general shape of the mean profile is probably substantially correct. However, the magnitudes of the mean cross-components are in doubt to some extent, although an idea of the degree of inaccuracy involved may be obtained by considering the mean component at the level of the jet axis. The mean value at the jet axis equals − 8·1 kt and the standard error of our estimate of the mean is 3·5 kt, implying a 95 per cent probability that the mean value (− 8·1 kt) is correct to within 7 kt; thus it is highly probable that the mean value is at least a negative quantity or, in other words, that there is a small cross-component in the direction from right to left, looking downstream.

Fig. 3 demonstrates that the maximum cross-flow in the exit region is at the level of the jet axis and from left to right, looking downstream; it is across the jet axis in a direction opposite to that occurring in the entrance region. The differences between the profiles of the right and left sectors can be explained by the direction of the isobaric contours (geostrophic winds) in each of these sectors. The combined mean profile eliminates any bias due to a predominance of observations on one particular side of the jet axis.

That the mean profile of Fig. 3 is likely to be substantially correct is also indicated by statistical considerations. The mean difference between the transverse components at the level of the jet axis and 200 mb below is highly significant (mean difference is 12·2 kt and the standard error of our estimate of the mean is 2·1 kt). The mean difference between the cross-flow at the jet level and 100 mb above is significant at the two per cent level of significance (mean difference is 4·5 kt and the standard error of our estimate of the mean is 1·9 kt). Thus there is at least a 98 per cent probability that the differences discussed are real, so that the general shape of the mean profile is well established. The maximum mean cross-component, which occurs at the level of the jet axis, is equal to 9·7 kt, and the standard error of our estimate of this mean is 2·4 kt. Hence there is approximately a 95 per cent chance that the true mean cross-component lies between 4·9 and 14·5 kt; it is at least reasonably certain that the mean component is positive, i.e., from left to right, looking downstream.

The small positive mean components in the central region of the jet (see Table 1) are not thought to be significant. They can be accounted for by the fact that the majority of wind observations in the central part tended to be nearer the exit than the entrance region – which is to be expected bearing in mind the greater frequency of occurrences of jet exits compared with jet entrances near the British Isles.

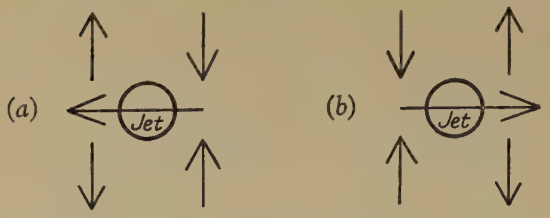

Figure 4. The probable circulation shown schematically in sections through (*a*) entrance and (*b*) exit to jet stream (looking downstream).

The transverse components given in Figs. 2 and 3 would be consistent with the existence of circulations in opposite directions above and below the jet axis, as indicated in the schematic cross-sections of Fig. 4. The circulation would be right-handed above and left-handed below at the entrance and the reverse at the exit. However, there is no direct evidence for the vertical branches of the circulations and the observations could almost equally well be explained by the upper and lower circulation alone. In view of the recognized temperature distribution both circulations would be 'direct' thermal circulations at the entrance and 'indirect' at the exit.*

4. The mean pressure of the jet axis in the entrance and exit regions

The pressure level of the jet centre was taken as the pressure level of maximum observed wind speed in the vicinity of the jet stream. Some additional wind observations were employed in this section since it was not necessary that they should reach 100 mb above the jet axis, as in Section 3. The results are summarized in Table 2.

TABLE 2. MEAN PRESSURE (mb) OF WIND MAXIMA TOGETHER WITH THE STANDARD ERRORS OF THE ESTIMATES OF THE MEAN VALUES

Region		Mean pressure (mb)	Standard error	No. of observations
Entrance	left	298	9	26
	right	259	8	23
	combined	279	6	49
Centre	left	295	6	48
	right	284	7	51
	combined	289	5	99
Exit	left	314	11	31
	right	298	10	32
	combined	306	7	63

Table 2 shows that the mean pressures of the wind maxima observed in the left sectors are generally greater (i.e., mean height lower) than those of the corresponding right sectors. This is not unexpected since the tropopause, near which the wind speed is normally a maximum, is generally at a lower elevation to the left and at a higher elevation to the right of jet streams. However, combining the observations in the left and corresponding right sectors (e.g., combining left and right entrance observations) should give

* A 'direct circulation' is one in which the vertical motion is upwards in the warm air and downwards in the cold air. The reverse type of circulation in which the vertical motion is downwards in the warm air and upwards in the cold air is called 'indirect.' A direct circulation is in the same sense as natural convection and converts potential into kinetic energy: an indirect circulation is opposed to convective overturning and reduces kinetic energy.

reasonably accurate values for the mean pressures of the jet axes in the entrance, centre and exit regions. The mean pressures of the jet axes are 279 mb, 289 mb and 306 mb in the entrance, centre and exit regions respectively. The mean pressure of the jet axis n the entrance region is less than in the exit region by 27 mb, i.e., the mean height of the entrance is about 2,000 ft greater than that of the exit.

In none of the cases used in the analysis was it possible to obtain height observations of the entrance and exit regions of the same jet, so that the mean pressures found are based on different jets. Thus the obvious interpretation of the mean pressure difference – namely that jets tend to descend slightly along their length downstream – is not necessarily correct.

Berggren *et al.* (1949) state that the height of the jet axis is lowest at the bottom of an upper trough and tends to rise upstream on the rear side and downstream on the forward side of the trough. They quote as evidence for this the height variation of the wind maxima at Larkhill during a period of several days when marked troughs and ridges moved eastwards; but the evidence cannot be regarded as entirely satisfactory for Larkhill was not situated in the same position relative to the strongest flow throughout the period. A similar idea is implicit in a paper by Palmén and Nagler (1949). On the assumption that there is in fact a tendency for the height of a jet centre to fall along the jet axis from north to south (on rear side of upper trough) and to rise from south to north (on forward side of trough), it was thought that the difference between the mean pressures at the jet entrance and exit regions in the present study might be due to a predominance of jets with a north-south component; but this explanation does not appear to be valid, since north-south and south-north jet streams were about equal in number in our statistics.

Another possible explanation of the mean pressure difference between the entrance and exit regions might be that the jet axis tends to rise with time on moving east across our area; for the jet-entrance statistics are likely to refer to later stages in the life history of the individual jets than do the exit region data.

It is clear from the above discussion that a conclusive interpretation of the mean pressure difference between entrance and exit regions cannot be given.

Acknowledgments

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The mean temperature of central England, 1698-1952

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Summary

A table of monthly mean temperatures representative of the English Midlands has been constructed for the period 1698-1952. From 1815 it is derived from the average of the 'Radcliffe' (Oxford) and 'Lancashire' monthly means. From 1771-1815 it has been built up by averaging the departures, or anomalies, for each month at a number of inland stations whose records are long enough to be bridged into the years 1815-1840; from 1771-1798 the values thus obtained are reinforced by direct values based on the long Midland record at Lyndon. Before 1771 the existing Edinburgh, Greenwich and Lancashire means are supplemented by further direct values based on Lyndon and Exeter. For all years previous to 1752 the Old Style calendar months have been rectified to the New Style, and the direct values derived from early MS. journals in London, Plymouth, and the Midlands have been carefully collated back to 1728, and extended by means of other short records from Halifax, London and elsewhere back to November 1722. Finally, a direct reduction of Derham's Upminster record, 1699-1706, has been used as a basis for the earliest years. For the intervening years estimates are provided from a consideration of the Utrecht reductions, supplemented for 1713-1722 by non-instrumental observations of wind and weather in England. Some corroboration of the values so obtained during the eighteenth century is provided; it is hoped that the table can serve as a general standard for studies of English temperature variations until such time as something better can be provided.

1. Introduction

Long-period tables illustrating the vicissitudes of a temperate climate have an evident value. In recent years investigations of the glaciers of north-west Europe go to show that the maximum postglacial advance of the remaining glaciers began at some time after 1300 and culminated within the last 200 years. The minor retreats and advances of the past century or thereabouts can be correlated with the fluctuations of the climatic elements since the time of establishment of standardized meteorological observations under national auspices. Botanical evidence moreover indicates that the north-west European climate has undergone several minor recessions and ameliorations since the Climatic Optimum, with recession on the whole predominant. Yet the amplitude of the temperature fluctuations indicated by the vegetation is little greater than that which may prevail from decade to decade at the present day. It thus becomes evident that further study of the reasons for the minor fluctuations of the past three centuries may provide the key to many of the events of the postglacial period, and by extension, to those of the Ice Age itself.

Further extension in time of our knowledge of yearly, seasonal and monthly variations is indeed desirable but with the diminishing precision of instruments and technique the difficulties become very great. Methods of approximation must be resorted to, most notably in England where, despite our very long scientific tradition, almost all our observation before 1841 was dependent on amateur effort, so that widely scattered records of diverse length and accuracy provide endless problems. It is the purpose of this paper to attempt to lay down a table of monthly means comparable with that provided by the late Dr. Labrijn for Utrecht in Holland which goes back to 1706, and indeed to provide some slight extension. Labrijn (1945), however, was able to assemble by straightforward methods a succession of overlapping records kept by different observers, not very far apart, and in a country of uniform relief. The English records offer a more formidable problem.

Figure 1. Location of the principal stations referred to in the text.

List of principal records, used in compiling a 'Central England' table

(A)	1815-1952.	Monthly means directly derived from Oxford (Radcliffe) and Lancashire; the latter based on 4 to 7 north-western stations reduced to a common standard.
(B)	1799-1814.	London (Royal Society), London (Strand), Salford (1807 on), Stroud, Derby, South Kyme with Lancashire and Edinburgh, all bridged into (A).
(C)	1793-1798.	Above; supplemented by Lyndon (Rutland) directly reduced to 'outdoors.'
(D)	1786-1792.	Above; lacking Derby and Stroud.
(E)	1771-1785.	The same, together with Stroud, also Greenwich reductions.
(F)	1764-1770.	Direct numerical values based on Edinburgh, Greenwich, Lancashire and Exeter, appropriately corrected to the Oxford-Lancashire means (called here "RL").
(G)	1754-1763.	Lyndon (Rutland) monthly means from cool-room readings; Exeter (1755 onward) and Lancashire, corrected to RL and given less weight.
(H)	1753.	Lyndon only; chance of gross errors diminished by rough Lancs. and London data.
(K)	1748-1752.	Lyndon; Plymouth reduced to probable outdoor values and to RL; London (St. John's Gate) for 1751-52 : London (Jurin) to 1750. All adjusted to the New Style calendar months.
(L)	1745-1747.	London (Jurin); Plymouth; Lyndon (mean of extremes only).
(M)	1728-1744.	London (Jurin's earlier location, reduced to outdoor rural exposure and to RL); Plymouth; Lyndon (mean of extremes) 1736 onward; Southwick (Northants), outhouse observations from 1727-1739 reduced to outdoors, New Style and RL. Edinburgh 1731-1736 as partial check.
(N)	1723-1728.	MS. records as described : London (Hauksbee), Halifax, Plymouth and others, overlapping into (M).
(P)	1707-1722.	Estimated from Utrecht, except for a few months in 1707 and 1722.
(Q)	1699-1706.	Upminster; outdoor shade readings at fixed hours reduced to °F., New Style, monthly means for the Midlands. Estimates for 1698 added based on Upminster and Shropshire journals.

2. The choice of long-term records on which to build; some existing tables

Two major sets of monthly means for London back to 1763 and for Edinburgh back to 1764 are well known. Some comment on these is necessary.

The London data were standardized by normal 'bridging' through overlapping series on to the Greenwich hourly observations which began in 1841. These have now

come to an end. While they are said (Jones 1939) to be not absolutely impeccable on account of slight variations in radiation effects from buildings which probably affected the ' Glaisher stand ' on which the readings were taken, they have provided, nevertheless, a longer and more satisfactory basis on which to build than any other London station. They were carried back to 1771, principally by means of the Royal Society observations (1774-1780, 1787-1843) which were carefully reduced by Glaisher (1849, 1850) and supplemented from 1814 onward by additional Greenwich observations and others from Epping, until the official Observatory station began recording late in 1840. A second reduction of the London data, back to 1763, is available (Buchan 1893); the chief component before 1822 being derived from Thomas Hoy's observations, which from August 1782 onward were made at Sion House. The two series differed appreciably in the earlier years and were integrated by Dr. C. E. P. Brooks to form the well-known table printed by Sir David Brunt (1925) in his early paper on periodicity; the standard being that of the Greenwich hourly means. This will henceforth be referred to as ' Greenwich.' Hoy's journals at Sion House have also been used by Drummond (1943) who has essayed the problem of carrying back a table of monthly means representative of Kew Observatory.

I have been led to doubt the validity of those parts of the early ' London ' tables which are dependent on Hoy, on account of the phenomenally high figures given during the years 1782-1786 and here and there elsewhere; for example, the quite extraordinary mean temperature of 48·1°F or thereabouts given in the ' Greenwich,' ' Kew ' and ' Buchan ' tables for January 1796, as well as the mean of 73·7°F given for July 1783. These values are meteorologically impossible if we accept the published means for other NW. European stations in those months. Moreover, the trend of the annual means shown by ' Greenwich ' about 1782-1807, does not accord well with that of Edinburgh or Utrecht. Scrutiny of Hoy's published means gives the impression that at times the mercury column in his thermometer became broken and there is evidence that this occurred elsewhere, for example, in the irregularities of the Derby record (1793-1838) during the earlier years. On the whole therefore I have felt that, before 1820, greater confidence should be placed in the trend, if not the actual reduction, shown by the ' Royal Society ' observations. These were kept at Somerset House and the best instruments may be presumed to have been used. It must be agreed that some criticism of the way the observations were then kept was made by John Dalton (1819) but soon afterwards this appears to have been put right (Dalton 1840) and, as the observations appear to have been maintained in the same circumstances from 1787 to 1843 there is a valuable overlap with the existing Oxford (Radcliffe Observatory) record which began in 1815.

The exceedingly thorough reduction of the more distant Edinburgh series by Mossman (1897, 1900) remains a landmark in British climatology and it is with diffidence that one may point to some slight likelihood of error which might possibly arise on account of the uncertainty of early thermometer corrections; Mossman's careful integration of different overlapping records is, however, very reassuring.

The more recent Lancashire table, standardized by the present writer (Manley 1946) is quite independent of the others, and is based on overlapping series of observations together with direct reduction with allowances for site, from never less than two north-western stations, back to 1784. For still earlier years back to 1753, the monthly means should be regarded with greater reserve.

The Radcliffe Observatory series at Oxford (Knox-Shaw and Balk, 1932) beginning in 1815, appears to be more reliable than any other in southern England, on account of the methods of reduction, the relative lack of the urban effects associated with London, and the careful nature of the observations throughout.

3. Some later 18th century records: reasons for adoption of a 'central England' standard

Before 1800 one of the best series of daily observations of temperature was kept from 1736-1798 at a very representative site in the East Midlands, by Thomas Barker at Lyndon in Rutland, and the part from 1748-1763 has recently been discussed and reduced by the present writer (Manley 1952a). His extant manuscript diaries give daily observations from Mar. 1748 to Aug. 1763, and Jan. 1777 to June 1789. Monthly summaries from 1771-1798 were published in the *Philosophical Transactions*, and they include the results of both indoor 'cool-room' observations and outdoor 'shade' observations, twice daily. A scanty summary of Barker's earlier observations (1736-1748), giving monthly extremes, was published by Short (1750). In the Society's Library there is also an Exeter diary (S. Milward, 1755-1774) giving daily readings at noon of a thermometer indoors in a 'cool-room' following the prevailing earlier eighteenth-century fashion. While it is capable of rough reduction to outdoor means by the same method as that for Barker, it is not very satisfactory; careful comparison with other stations points either to minor changes of location, or of the thermometer; one cannot be certain. Nevertheless, these two records appear to be the only surviving sources of daily observations for the year 1762 in England. It should be added that valuable introductory notes and summaries for several of these early MS. series were made by Mr. C. E. Britton and are available in typescript in the Society's Library.

A later Midland record kept in or close to Stroud by a Dr. Hughes, covers Jan. 1771-Jan. 1774 and Jan. 1775 to Mar. 1813 (MS., three volumes, in the Society's Library). The figures have never been reduced, and I have with some labour taken out the approximate monthly means for thirty-five years. Three changes of exposure, rather variable observing hours from month to month, and the prevailing omission of one or two days each month do not render this a very congenial series, but the morning observations for 1775-1785 are nearly complete, and also twice-daily readings from 1803-1812. From these one can compare the means for successive months of the same name. Derby (1793-1838) with, as a rule, thrice-daily outdoor readings was kept and beautifully tabulated by T. Swanwick in an MS. volume now in the Society's Library, but the results are unfortunately subject at intervals to large and persistent thermometer errors, easily detected against other contemporary observers.

The existence of these four, and notably the long Barker series in the East Midlands, led me to think that the best method of standardizing any of the scattered earlier English records would be to try to bring them all to a comparable standard, namely a 'Central England mean' represented nowadays by 'Radcliffe' (i.e. Oxford) plus 'Lancashire' divided by two. Let us call this, henceforth, 'RL.' For it will be evident that a direct extension of the Oxford tables could not well be effected as no reliable adjacent stations existed. Similarly, a direct extension of the 'Lancashire' tables by interpolation from places as far distant as Lyndon would be far more likely to error.

For if we study the normal pattern of the isotherms we soon observe that, whether the month be excessively cold or extremely warm, the departure of the mean temperatures from the normal throughout the Midlands is likely to be well represented by the departures of RL, i.e., $\frac{1}{2}$ (Oxford + Lancashire). It must be emphasized that 'Oxford' implies the corrected Radcliffe monthly mean which differs a little from the average of the maxima and minima; the corrections are discussed (Knox-Shaw and Balk 1932) in the Appendix to the 'Radcliffe Observations' for that year.

Inasmuch as they are all Midland stations, better use of Lyndon (1736-1798), Stroud (1771-1813) and Derby (1793-1838) could be made by relating them to RL than to any of the other long series. In order to be sure of the merits of RL as a central England

mean, I have taken out the means for every month from 1931-1950 for a number of stations, e.g., Cheltenham, Rugby, Oundle; the results are very reassuring. On inspection it also appears that the monthly means for RL for 1906-1935 bear a close resemblance to those at Shrewsbury and to Giggleswick + 2·0, Buxton + 3·2, Welshpool + 0·3, Marlborough + 0·8, Huddersfield + 0·9, Cullompton − 1·2, Hereford − 0·2, Macclesfield + 1·4. The Meteorological Office mean for District 4 ('Midland Counties') is 0·4 lower in December-January and 0·4 higher in July-August; over the year it is nearly 0·1 below RL for the same period. RL therefore represents the probable 24-hour mean which would derive from observations at a site of intermediate character between 100-150 ft above sea level in Shropshire, south Cheshire or Worcestershire, i.e., the west Midlands. More easterly Midland stations such as Nottingham, Worksop, Wakefield and Cambridge, and even Copdock near Ipswich, were slightly warmer in summer and cooler in winter. All this, of course, is to be expected; nevertheless the consistency is agreeable and leads to the conclusion that the average of the anomalies presented by the monthly means at any pair of Midland stations, lying respectively towards the north and towards the south, should be quite close to that presented by RL. Incidentally the monthly means at London stations present anomalies in good general agreement, considering the miscellaneous urban effects, with other south Midland stations such as Oxford. Further, the interesting fact emerges that, after test over a number of years, the mean of ' Edinburgh ' and ' Greenwich ' lies close to that of RL, taking ' Edinburgh ' to be the location at 250 ft altitude to which Mossman's reductions apply, while ' Greenwich ' is the mean hourly value to which the long tables of London data have been standardized by Dr. C. E. P. Brooks. Hence the departure of (E + G)/2 from normal in any given month, i.e., the mean of the E and G anomalies, appears to give a fair approximation to the ' Midland anomaly,' that is the departure of RL from normal in the same month.

Hence the problem has been, to derive a ' central England mean ' for each month from one or more records, sometimes well within the Midlands, sometimes towards the periphery. The further towards the periphery, the greater the chance of error, unless the record in question could be bracketed with one on the opposite side. If, to quote an extreme case, one estimates the ' Midland anomaly ' for any given month from that prevailing at Plymouth Hoe far to the south-west, errors of up to 2°F may be expected. For in any case the anomalies at a coastal station tend to be less, in extreme months, than those inland. Hence the Cullompton (Exeter) anomalies accord considerably better with those of the Midlands, than do those at Plymouth Hoe.

In view of the fact that records of various sorts in London go back to 1723 with only minor gaps about 1753-4 and in 1762, it may be asked why the existing London tables should not be extended without further ado. Unfortunately, really good ' bridges ' or overlaps are lacking; and further, the varying size of London makes it extremely difficult to decide how to standardize the earlier records; thirdly, the imperfections about 1783 previously mentioned should be eliminated.

4. Compilation of a ' Midland ' mean before the Oxford tables begin

The assemblage of the monthly means for RL back to 1815 thus presents no difficulty. But the backward extension of Midland means through the Napoleonic wars in order to overlap the Barker (Lyndon) means, which end in 1798, is much more tricky. It has involved the careful scrutiny and test of the following records :

(1) Braithwaite near Keighley, 1799-1857 (Shackleton). 8 hr readings, indoors before 1809. Reluctantly discarded owing to early irregularities and doubtful exposure (MS., Keighley Library).

(2) York, 1800-1824. 8 hr readings. Very large thermometer errors at intervals especially about 1807-1808. Of little utility. (Gray's record; Meteorological Office Library).

(3) Stroud, 1803-1812 twice daily; tolerably complete, quite useful, consistent exposure. Also 1793-1812, morning observations only. (Hughes, R. Met. Soc. Library).

(4) Salford, 1807-1818; extremely urban, but consistent (Hanson, scattered in contemporary journals).

(5) Derby, 1793-1838. Earlier period subject to long spells of erroneous readings of constant magnitude, when compared against others, but tolerably useful (Swanwick's record, R. Met. Soc. Library).

(6) South Kyme (near Sleaford, Lincs.). 1800-1869 (summary at Meteorological Office Library). Apparently 8 hr; many details lacking, but after test against Royal Society, 'Oxford and Lancashire' for 1815-1824, the figures appear to agree very well, allowing for exposure and early hour of reading; hence the departures of the monthly means back to 1800 from that of 1815-1824 are reasonably trustworthy. Although relatively close to the East coast, this record is quite fairly representative of the 'East Midlands.'

(7) Mongewell (near Wallingford) 1773-1823. Presents great difficulty owing to irregular hours, but useful as an independent check for individual months. (Royal Society Library MS.).

(8) Strand, London (Cary's record, 1786-1846, *Gentleman's Magazine*). Discussed below. In addition, the other London series (notably the 'Royal Society') already mentioned, also 'Edinburgh' and 'Lancashire.'

The primary assumption made is that the overall Oxford (Radcliffe) means for 1821-1840, and the overall Lancashire mean for the same twenty years, are acceptable. The 'Royal Society' means, reduced by Glaisher, provide one of the overlaps; and the departure of the mean for every month back to 1787 from the mean for 1821-1840 was taken out and entered. Similarly, the departure of every month at Edinburgh from the mean of 1821-1840 was taken out and entered and compared with that of London. The Stroud means, 1803-1812, were standardized on to Oxford 1821-1840, through the Royal Society overlap, thus providing a further series of Midland anomalies for each month. A second Stroud set of means, 1793-1812, was calculated out using the morning observation only, and by the like bridging process a second series of anomalies for each month was carried back to 1793. The Derby anomalies for each month back to 1793 were taken out by overlapping against RL for 1815-1824. The Salford anomalies, back to 1807, were taken out against RL for 1815-1818, a relatively short but useful series covering the gap between the end of the Stroud record (February 1813) and 1815 when the Oxford observations began.

Nevertheless, a further check appeared desirable in view of certain apparent inconsistencies in the Royal Society and Greenwich reductions and the doubtful features attached to both Stroud and Derby. A completely independent series of observations in London was kept by Cary, the well-known instrument maker, in the Strand. It is extremely urban, with two observations daily (8 hr and noon) but agreeably long and apparently very consistent from 1786-1846; there may be a slight error for a few months in 1818. It has never been reduced as a whole to monthly means. With much labour, and aided by several friends I have taken out the means for the 40 years 1786-1825. In order to diminish the evident radiation effects, I have given treble weight to the 8-hr observation in comparison with that at noon. I then took out the departures for each month, 1800-1814, from the mean for 1815-1824 for the three stations : Cary, South Kyme and Lancashire; the mean of the three anomalies has been regarded as representative

of the Midlands. For each month it provides a nearly independent check on the anomaly derived from Royal Society plus Stroud plus Salford plus Edinburgh plus Derby plus Lancashire described earlier. For the period 1786-1799 the mean of the two anomalies (Cary and Lancashire) has been used as a check on the Midland anomaly otherwise obtained.

For 1771-1798 we have the Lyndon (Rutland) record kept by Barker. Throughout this period he maintained indoor (i.e. cool-room) and outdoor readings; but in 1786 he broke his outdoor thermometer, and after replacement the outdoor means differ from those indoors by an average of 1·7°F less than they previously did. Comparison with overlapping series elsewhere leads me to the conclusion that his outdoor thermometer read too low before 1787 and hence that the mean temperatures derived from the indoor readings are more likely to be correct, and accordingly I have reduced them all on the assumption made in my previous short paper on Barker's records (Manley 1952a). For the years 1771-1798, therefore, we have a series of approximate monthly means for Lyndon which is quite independent of any other series. Having obtained them it was agreeable to find that the departures from normal (based on the probable value, derived from Oxford, for Lyndon, 1821-1840) agreed very well with those provided by means of the above bridging, that is by averaging the Royal Society, Edinburgh, Lancashire and Stroud anomalies previously derived and checked with the aid of Cary back to 1786. With the aid of Lyndon, Lancashire, and to some extent Stroud as a check on Lancashire, I have thus calculated the probable value of the Midland anomaly for each month back to 1771; and for the year 1774-1781 it was also possible to use the Royal Society observations again, and to check the approximate value for each month of the Midland anomaly by working out that resulting from ½ (Edinburgh plus Greenwich) for the same month.

Lyndon, being well in the Midlands, provides a good index of the probable value of the monthly mean temperature in the region represented by RL for the years 1748-1763; the corrections, Lyndon to RL, are quite small. For the intervening period 1764-1770 I have taken out the direct mean temperature, rather than the anomalies, from a consideration of the values given by Lancashire, Greenwich, and Edinburgh in conjunction with the rather approximate means resulting from the reduction of the Exeter record kept by Milward, 1755-1774. For 1753-1759 the Lancashire values supplement those derived from Lyndon but I have given them less weight. This means that the value of RL for each month is principally dependent on the stations lying eastward and northward; far to the south-west, however, the Exeter record provides at least a minor check back to 1755. We have seen already that over a thirty-year period, the monthly mean temperatures at Cullompton, near Exeter, differ from those of the Midlands by a remarkably constant amount; so that for individual months the estimates of the departures of the Midland mean from normal are not likely to be too bad, even from so distant a region as Devonshire.

It may be mentioned in passing that Gilbert White kept records at Selborne from 1768-1793; I have examined his MS. as a south-country inland set for that period would have provided useful reinforcement; but there are far too many gaps due to absence. There is, however, a very long and hitherto unreduced set of observations at Mongewell, 13 mi SE. of Oxford, among the MS. in the Royal Society's Library; daily observations of a well-shaded thermometer are almost complete from 1773-1823. It is, however, a very difficult set to reduce owing to a great lack of consistency in the hours of observation and possible variations in the exposure. I have, however, taken out a number of ' sample ' months for comparison, more particularly the very hot or cold months such as July 1783, 1794, 1808 and 1818, and January 1776, 1780, 1795, 1814. The resultant check on the general reductions otherwise made has proved remarkably reassuring.

Reduction of the monthly means is necessarily complex owing to the need to allow for both distance and reliability of the several sources of data. Illustrations are given below :

TABLE 1. ILLUSTRATION OF THE REDUCTION FOR TYPICAL MONTHS

January 1797

Departures of the Monthly Mean (°F) from that of 1821-1840 :

(A) Royal Society = − 0·7
(B) Greenwich = + 3·7 (i) $\frac{A+C}{2} = +1{\cdot}9$ (' Rough Midland anomaly ')
(C) Edinburgh = + 4·4

(D) Lyndon = + 0·8
(E) Lancashire = + 1·7 (ii) $\frac{1}{2}\left(\frac{A+C}{2}+D\right) = +1{\cdot}3$ (' Average of above with Lyndon ')

(F) Derby = (+ 2·2)
(G) Stroud = (+ 2·4) (iii) $\frac{1}{2}\left[\frac{1}{2}\left(\frac{A+C}{2}+D\right)+E\right] = +1{\cdot}5$ ('Average of S. and E. Midlands and Lancashire')

Greenwich (B) above not used as it is persistently high about this period.
Mean value of RL for 1821-1840 for January = 36·7
∴ Probable value for January 1797 (using (iii)) = **38·2**

Check on the above value

Departures of the Monthly Mean from that of 1815-1824 :

(H) ' Cary ' (London) = + 1·4
(K) Lancashire = + 1·2
(H + K)/2 = 1·3

Mean value of RL for 1815-1824 for January = 37·1
∴ Probable value for January 1797 = **38·4**
Adopted value, taking account of the more doubtful (F) and (G) above = **38·3**

July 1776

The above stations give the following departures :

(A) = + 1·4, (B) = + 0·9, (C) = + 0·3, (D) = + 0·3, (E) = + 1·0, (G) = + 0·4

Greenwich is here used, with the result :

Probable anomaly for S. Midlands $= \frac{1}{2}\left(A+\frac{B+C}{2}\right) = +1{\cdot}0 = Y$
Probable anomaly for S. and E. Midlands $= \frac{1}{2}[Y+D] = +0{\cdot}7 = Z$
Probable anomaly for W. Midlands applicable to RL $= \frac{1}{2}[Z+E] = +0{\cdot}8$

Mean RL for 1821-1840 for July = 60·6
∴ Probable value for July 1776 = **61·4**
The Stroud value, although doubtful, is in sufficient agreement as a check.

5. SOME FURTHER PROBLEMS PRESENTED BY OBSERVATIONS BEFORE 1753

In England daily readings of thermometers exposed to the air were first maintained, as far as is known, by Robert Hooke in 1664. Following his advocacy of the need for such studies, during the later part of the seventeenth century daily observations of pressure, temperature, rainfall and wind direction were begun in a number of places and some of the resultant ' registers of the weather ' have been preserved. Unfortunately the surviving records of temperature are not only scattered but much interrupted. Some of these early manuscript records were sent to the Royal Society; among these the first really useful set of daily observations covers the years from 1699-1706. They were kept by William Derham at Upminster, whose rainfall measurements (1697-1716) are already well-known. As yet I have not devised a way of incorporating any observations of earlier date.

In 1723 Jurin, who was then Secretary of the Royal Society, published a short paper

in which he advocated the maintenance of regular daily observations and invited contributions. As a result he received quite a spate of tables of carefully maintained daily readings, sometimes reduced to monthly means, from various parts of the country. For our knowledge of eighteenth century weather we are particularly indebted to Jurin's medical colleagues with whom he entered into correspondence; of these, perhaps the most noteworthy was Huxham of Plymouth. In his subsequent *Essay on Fevers*, which went through several editions, Huxham added careful summaries of the weather of each month, and in particular he gave means and extremes of his temperature readings which cover the period 1728-1752, and were kept by 'Jurin's method.'

Jurin himself, while in practice in London, kept a daily register of his observations following the general method which he had advocated in his 1723 paper. The thermometer he says 'should preferably be one made by Francis Hauksbee and should be exposed in a fireless room where no sun can penetrate.' As a result of this the majority of English records of temperature before 1755 are kept under circumstances which are not easy to interpret or standardize. The so called 'Royal Society' thermometers made by Hauksbee and copied by others used a scale on which, according to Martine (1740), zero was equal to 88°F while 'between 78 and 79' gave freezing-point. How the upper point was established on these spirit thermometers is not clear, neither is it evident that they were strictly comparable. For Barker used a 'Royal Society's pattern' thermometer made by Patrick on which freezing point lay at 76·5. On one occasion Jurin put his own instrument out with its bulb in the snow where it read 78°; it is not quite clear that the snow was melting at the time. The bulbs were spherical and frequently upwards of an inch in diameter, so that we may agree that they would not be at all responsive by modern standards; moreover, there is some evidence that the scale reading of the freezing point might change with age. Lastly, while readings twice daily were thought desirable, and the time of observation was often noted, the actual time of reading varied a good deal. With indoor cool-room observations, however, the effects are not too serious (late editions of Martine also give slightly different conversions).

Nevertheless, continuity of daily recording in cool rooms was maintained by several observers for long periods, so that with suitable methods of interpretation an index of the vicissitudes of the monthly mean temperature can undoubtedly be produced. The problem is, how to standardize such scattered individual records and how to build them into a temperature series which can be brought down to the present day.

6. Before 1753: the rectification of the calendar months

There is a further problem with regard to all English records before September 1752; existing monthly means refer to the Old Style or Julian Calendar and as the stagger amounts to eleven days they are far from comparable with those of the New Style or Gregorian months. It so happens that from 1729 to 1747 there is only one set of daily observations in manuscript; from March 1747 to September 1751 there is a second incomplete daily record; from March 1748 onward to the calendar change in September 1752 there is a third but with a nine months' interruption. Hence for the intervals Mar. 1729-Feb. 1747 and Oct. 1751-Aug. 1752 we are oddly enough dependent on a single set of daily observations, in order to make the translation of Old Style to New Style monthly means. However, so long as we are dealing with English Midland stations the relationship between Old Style and New Style means for any given month is probably very closely represented by that registered at any other inland station. There is no way of avoiding such an assumption if we are to devise New Style monthly means for England for comparison with Holland, where the New-Style calendar was adopted in 1700, or with our present-day monthly means.

Barker's extant MS. journal begins in March 1748 and a straightforward series of monthly means for the Old and New Style months was soon compiled. There is, however, a gap Oct. 1751-Aug. 1752, but during these months daily readings of a thermometer at noon, at St. John's Gate in London, were published in the *Gentleman's Magazine*; unfortunately, excluding the Sundays. Nevertheless, the relationship between the Old and New Style means can be closely estimated for that period.

To carry back the rectification further, we have a simple MS. journal kept by Jurin, Aug. 1728-Mar. 1750, giving daily readings on his own system, that is within a cool room in London. Still further back we have full tables of Hauksbee's daily observations in London at Crane Court (July 1723-Dec. 1728); Nettleton's at Halifax, 1724-1727; and Stukeley's in London (Nov. 1722-Feb. 1725). Lastly, the daily observations kept by Derham cover the years 1699-1706; his non-instrumental data for 1698 enable estimates for that year to be added. These are the principal MS. sources and, together with some minor MSS. are all preserved in the Library of the Royal Society. None of them have hitherto been reduced to modern standards.

It must of course be agreed that the difference between the monthly means for any one New Style and Old Style month, if derived from observations in London, is not likely to be precisely the same for other parts of the country; but from a few comparisons the difference does not appear to be at all serious so far as the Midlands are concerned. In general, of course the New Style months are cooler in spring and warmer in autumn than those of the Old Style. Casting up the two sets of means from these numerous manuscripts over a period of 38 years has been a very prolonged task.

7. The derivation of central England means from these early records

Barker's MS. takes us back to March 1748. We now have to make use of Huxham's fine series of monthly means of temperature based on twice-daily observations of a Hauksbee thermometer ' on Jurin's method ' at a house in the sheltered older part of Plymouth (Huxham 1752, 1770). Nearly three earlier years with his daily readings are available in MS. (Mar. 1724 to Dec. 1726). Although the 1724 readings are of little use, as he used another pattern of thermometer, Huxham provides us with the monthly extremes, and careful notes of the frequency of snow and sleet enable us to judge very well the overall mean temperature of the winter half-year, November-April, during the decades in question; compare my earlier notes on the subject (Manley 1948, 1952a). Huxham's rather elegant Latinity is also not unamusing. Comparison of the ranges shown by morning and afternoon temperatures and monthly extremes leaves no doubt that the place of exposure was quite well ventilated.

I have reduced Huxham's indoor monthly mean temperatures from the Old to the New Style using the comparison provided by the Jurin observations in London. I have then reduced these New Style indoor means directly to outdoor equivalents on the Fahrenheit scale, on the same general principles as I adopted for Barker's observations (Manley 1952a). The resultant overall annual mean for Plymouth for 1748-1752 can be compared against the Barker Midland mean for the same years. Huxham's overall mean temperature for the winter months, 1728-1752, must be very close to that of the present day to judge by his overall frequency of snow (Manley 1948); the temperatures of his extreme months, e.g., July 1733 can be placed close to the highest means likely to be observed today; and the temperatures he reports for extreme days, hot or cold, can likewise be plotted against today's extreme values. Some allowance has been made for the fact that Plymouth town is more sheltered than the Hoe.

Having thus estimated the Plymouth outdoor means for the New Style months for the years in question, the normal differences, ' Plymouth-RL,' have been applied, giving

a series of values for RL. These, however, are not likely to be at all accurate, as Plymouth lies a long way south-west and the temperature gradient to the Midlands varies a good deal especially in cold winter months (compare Jan. 1744, in Table 2).

Accordingly, I have derived a similar series of outdoor means, by similar methods, from Jurin's London observations between 1728 and March 1745. In that month he removed and the characteristics of his cool-room became appreciably different, in Lincoln's Inn Fields, from those at his previous house in Garlick Hill. One can compare the two London locations by reference to the overall sums of the monthly means at Plymouth. With regard to the later part of Jurin's record, May 1745 to March 1750, differences between successive months of the same name can be carried backwards from the Barker record which begins in March 1748.

We also know the mean of the monthly extremes of Barker's indoor thermometer for the months May 1736 onward; these were given by Short (1750). These provide a rough value for the monthly mean, and a check on that derived from the more distant London and Plymouth observations.

Midland observations of a more useful type were kept at Southwick, near Oundle, by George Lynn. He read a Hauksbee thermometer twice daily in an outhouse and published the means, for the Old Style months, for 1726-1739 (Lynn 1741). The range of Lynn's readings, daily, monthly and annual, indicates a well-aired outbuilding in which the thermometer responded well to outdoor variations. I reduced all these to *approximate* Fahrenheit equivalents (freezing point = 78° Hauksbee and 1·4°H = 1°F), which evidently differed but little from outdoor means; and also through Jurin to the New Style. Taking the years 1728-1739, an overall mean of all Lynn's observations could be compared with Huxham at Plymouth, and with Jurin at London; the approximate outdoor Fahrenheit equivalents of the means given for his warmest and coldest months could similarly be estimated. From these, a curve relating the means of his outhouse reading to outdoor equivalents could be drawn. Thus it became possible to eliminate error in the scale reading of the freezing point, to which Martine refers.

From 1728-1739, therefore, actual Midland observations reinforce those elsewhere; and the difference between these Southwick monthly means, and those representative of RL, has been considered to be 0°·2 greater than that between Oundle and RL at the present day, based on the difference in altitude.

At this stage, having derived a series of approximate values of RL for every month back to 1728, it was recalled that Mossman (1900) reduced an independent Edinburgh set of observations, 1731-1736, to modern standards by the best methods he could think of. Knowing the mean difference, Edinburgh − RL, it was agreeable to find that the differences between Mossman's reduced values and my own for the years 1733-5 were reasonably accordant with those prevailing in more modern times. Secondly, as the series of monthly means was steadily lengthened, very pleasing general agreement has been found with the Utrecht tables (Labrijn 1945). For example, the phenomenally warm September 1729, the hot July of 1733, the severely cold October of 1740 and the warm November of 1743 are all in admirable accord.

For the earlier years we have Hauksbee's own readings, twice daily, at Crane Court in London, July 1723-Dec. 1728. These provide a means of adjusting Lynn's Southwick means for 1727 to the New Style and carrying back the results of actual Midland observations for a further year (Lynn for 1726 is unreliable). For 1724-1727 Nettleton's observations from Halifax, again using Hauksbee's thermometer and the method advocated by Jurin, are exquisitely tabulated and averaged, but as indoor readings they appear to suffer more than most from lag; it is tempting to recall the solidity as well as the chill of the older stone houses of Yorkshire. Hence I have taken back the means to 1723

largely by repeated differencing of months of the same name, both in northern and southern England, from the values of RL derived for the years 1727 and 1728. Stukeley's MS. observations indeed take us back to November 1722, but here the problem arose of a different thermometer scale of unknown derivation; fortunately it could be bridged with Hauksbee's observations, and supplemented with a brief series of monthly means from Northumberland. All these MS. records have been gone through in detail, but regard for the exigencies of space and the likelihood of tolerance of complex arguments about remote events, suggests curtailment.

TABLE 2. ILLUSTRATION OF THE REDUCTION FOR TYPICAL MONTHS

		RL Adopted Mean
January 1756		
RL, based on Lyndon reduced to 'outdoors'	39·9	
RL, based on Lancashire	40·3	40·0
RL, estimated from Exeter	39·9	
January 1744		
RL, based on London (Jurin) reduced to °F, New Style and outdoors	35·8	
RL, based on Plymouth similarly treated	33·2	
RL, based on departure of Plymouth from mean of Plymouth 1748-1752, applied to Lyndon's mean for those years	33·7	34·5
RL, based on mean of Lyndon monthly extremes	35·5	
January 1730		
RL, based on London (Jurin) as above	37·6	
RL, based on Plymouth (Huxham) as above	39·3	
RL, based on Southwick (Lynn)	39·0	38·5
RL, based on departures of Plymouth from mean of Plymouth 1748-1752, applied to Lyndon's mean for those years	39·6	

Finally, we go back to the beautifully tabulated annual MS. summaries for Upminster, east of London, which were sent to the Royal Society by William Derham; the first of these, for 1699, was printed (Derham 1700). His thermometer, nearly three feet long, was exposed on a north wall and read thrice daily at stated hours. He records the freezing point on his scale as 82·2° (his 'degrees' were scaled in linear inches and tenths); the extreme values over eight years are 58 and 186. From a consideration of the probable extreme values over a series of years on a north wall at Upminster, which undoubtedly would bear a close relation to those in the old North Wall screen at Kew, I have interpreted Derham's scale. I have then reduced his observations, adjusted them for intermittent omissions, and corrected them for the hours of observation which vary from month to month, using the Kew corrections to a twenty-four hour mean. The result certainly looks very well. But it is finely supported by the fact that the annual mean thus derived for 1706 for Upminster differs from that at Utrecht by very much the same amount as we should expect today. No other test whatever is available, other than the frequency of snow in relation to the winter mean temperature over this period. This is also satisfactory. As the fluctuations of the monthly means at inland stations near London are in fair agreement with those in the Midlands, I have extended the RL means from the Upminster values using the normal monthly differences between RL and the average of six Essex and nearby stations today. The values so derived for RL however are smoothed to the nearest half-degree. Estimates for the year 1698 have been added, based on Derham's earlier non-instrumental record and on notes by a Shropshire farmer (abstract of T. Like's journal by C. E. Britton, R. Met. Soc. Library); as there is evidence that May 1698 was colder than any subsequent month of that name.

Unfortunately, no other English records exist, so far as is known, which allow us to bridge the gap 1707-1722, except for a few months in 1707 when some sketchy MS.

observations (Beighton's) from Coventry permit a very rough reduction. I have accordingly made estimates of the mean temperature of the Midlands, to the nearest degree, based on the Utrecht table in Holland. These, especially in cold winters, must be very approximate; in summer they are likely to be better. My estimates for each month during the years 1713-1722 have also been supplemented and adjusted from a consideration of the daily wind and weather, observed by G. Smith at Richmond near London (MS. summary, R. Met. Soc.). From these one can to some extent judge the probable incidence of the air masses in England by comparison with Holland. There is always the possibility that further observations of temperature may be found; but at present there seems little hope, and those who are curious about the trend during the early years of the eighteenth century must therefore be satisfied by the interpolated estimates below. The Northern Ireland diary (1711-1725) kept by Le Neve only contains temperatures from June 1722 onward, so that it too proves rather a disappointment.

8. Conclusion

This paper is long, and would be far longer if all the stages of a compilation which has taken nearly five years to complete were argued in detail. The principal link is through Oxford and Lancashire for 1821-1840; should these reductions be found to err, the earlier tables will need corresponding adjustment. But the reductions of still earlier MS. series through a consideration of the probable outdoor equivalent of indoor means seem to lead to good results. For in the first place the effects of varying readings of the freezing point on different thermometers are eliminated. Moreover, we have a useful test of the overall average of the winter mean temperatures for long periods in the eighteenth century through a critical study of the observations of snow frequency. To provide some confirmation of the resultant values of mean temperature, we may quote :

TABLE 3. Comparison of past and present snow-frequencies and winter mean temperatures

	Period	Average annual number of days with snow or sleet observed	Mean temperature °F, (RL) for November-April
Tonbridge and NW. Kent (Source of data : Hooker's MS Journal, R.S. Library)	1729-1765	13·0	41·1
North-west Kent at similar levels and by similar standards	1912-1938	(12·5)*	41·7
	1931-1950	12·0	42·0
Upminster (West Essex)	1698-1706	13·8	40·6
West Essex at similar levels and by similar standards	1912-1938	(12·0)*	41·7
	1931-1950	(11·7)*	42·0

* Approximate, based on available ' good-standard ' stations.

Although the Upminster data cover only nine years, which is a short period from the standpoint of a snow-frequency average, they do conform well with the expectation based on temperatures. Snowfall frequencies for London (1728-1749), Plymouth (1725-1752), Lyndon (1748-1763, 1777-1789) and Liverpool (1768-1793) are also satisfactorily accordant.

Scattered notes also exist in Barker's journals and elsewhere with regard to the thickness of ice on ponds and rivers, which confirm the remarkable intensity and length of the frost of 1740 by comparison with other severe winters. In that year, for example, the ice on the River Eden was reported to have attained 22 in. in thickness, and Hardraw

Force is said to have been completely frozen. Phenological records also have some value, e.g., for the springs of 1740 and 1750.

Moreover, the general pattern of evidence of extreme months, the range of variation they show, and the accordance with other NW. European records, all make for confidence in the reality of the values shown; and, while they must not be regarded as more than representative of the English W. or NW. Midlands and not precisely appertaining to any single location, particularly in winter anticyclonic months, it is doubtful whether any longer series, integrating our scattered observations, could be constructed which would at the same time be free from urban effects. Such tables of standardized monthly means provide also a basis for the discussion of the incidence of extremes, for the extraction of ' singularities,' and for the integration of minor scattered records. Before 1750 however statisticians should view the table with caution.

Discussion and illustration of the trends revealed by this table of monthly means and their relationship with other phenomena will be reserved for a later paper. In the light of current studies, the establishment of the comprehensive Table 4 has appeared to be a primary need. The January and July running means are however shown elsewhere on p. 193 of this issue of the *Journal*. A short table of extreme values is given below (Table 5), and decadal means are added in Table 6.

9. Acknowledgments

I have to express my very grateful acknowledgments to the Officers of the Royal Society for allowing me generous opportunities to consult the MSS. and other works in the Society's Library; and to Mr. Kaye, the Society's Librarian, and his staff for their help. I have also to thank the authorities of Lancing College for the loan of the early Barker journals, and the Librarians of the Royal Meteorological Society, of the Meteorological Office, and of the Keighley Municipal Library for further opportunities of consultation. I have to thank Mr. J. G. Balk of the Radcliffe Observatory for very kindly providing data for 1931-50 in continuation of his earlier published table. Lastly, I am most grateful to several friends for assistance, notably in reducing the Cary monthly means over the 40 years 1786-1825. To this the late Mrs. Margaret Anderson of Cambridge, with Miss Audrey Ashworth and Miss Mavis Rodgers, recent graduates from the Department of Geography at Bedford College, have generously contributed much time. The whole of the remaining calculations and reductions from the available material have been carried out by myself, and I shall be very ready to give details to those who may desire them, with regard to particular records or reductions.

TABLE 4. Reduced values (°F) of the monthly mean temperature representative of central England, 1698-1952

New Style or Gregorian Calendar throughout
(Figures in brackets must be regarded with reserve, for 1698 and 1707-1722)

	J	F	M	A	M	J	J	A	S	O	N	D
1698	(35)	(35)	(40)	(45)	(47)	(57)	(60)	(60)	(55)	(49)	(39)	(40)
1699	38·0	38·5	39·0	42·5	49·5	58·5	63·0	58·0	56·0	50·0	42·0	39·0
1700	40·5	37·0	38·0	48·5	53·0	55·5	59·0	59·0	55·0	48·0	42·0	39·0
1701	37·5	37·0	36·5	40·5	51·0	57·5	64·5	60·5	57·0	46·0	43·5	39·0
1702	40·0	44·0	43·5	41·0	51·0	56·0	59·0	61·0	57·5	49·5	40·5	38·0
1703	36·5	38·5	42·0	47·0	49·5	55·0	59·5	60·0	52·0	46·5	45·0	41·0
1704	36·0	38·0	41·5	47·5	51·5	57·0	62·0	61·5	53·0	47·5	43·5	39·5
1705	37·0	38·0	39·0	45·5	50·0	54·0	59·5	62·5	53·5	48·0	38·5	40·5

	J	F	M	A	M	J	J	A	S	O	N	D
1706	37·0	39·5	43·0	47·5	53·5	60·5	60·0	60·5	53·0	51·0	43·0	41·0
1707	(38)	(37)	(41)	(47)	(52)	(61)	(63)	(61)	(57)	(47)	(44)	(38)
1708	(43)	(39)	(44)	(49)	(51)	(57)	(58)	(64)	(59)	(47)	(45)	(37)
1709	(29)	(35)	(37)	(48)	(54)	(58)	(60)	(60)	(56)	(50)	(48)	(41)
1710	(37)	(38)	(43)	(44)	(53)	(58)	(58)	(60)	(56)	(49)	(46)	(46)
1711	(41)	(36)	(42)	(49)	(52)	(60)	(59)	(59)	(55)	(49)	(45)	(38)
1712	(37)	(40)	(41)	(46)	(52)	(59)	(61)	(58)	(55)	(49)	(43)	(42)
1713	(37)	(42)	(38)	(41)	(50)	(56)	(57)	(59)	(57)	(49)	(40)	(39)
1714	(39)	(42)	(41)	(45)	(50)	(58)	(64)	(58)	(55)	(51)	(43)	(39)
1715	(39)	(41)	(44)	(49)	(52)	(57)	(59)	(59)	(57)	(51)	(44)	(34)
1716	(28)	(37)	(40)	(48)	(51)	(57)	(60)	(59)	(54)	(49)	(42)	(37)
1717	(40)	(37)	(40)	(45)	(50)	(57)	(59)	(59)	(56)	(49)	(42)	(41)
1718	(34)	(37)	(42)	(46)	(52)	(58)	(63)	(63)	(58)	(49)	(43)	(40)
1719	(38)	(40)	(41)	(44)	(53)	(58)	(64)	(63)	(57)	(48)	(43)	(38)
1720	(40)	(39)	(39)	(45)	(52)	(55)	(61)	(57)	(56)	(47)	(43)	(42)
1721	(41)	(35)	(37)	(47)	(49)	(57)	(58)	(60)	(57)	(48)	(43)	(40)
1722	(39)	(41)	(43)	(46)	(51)	(57)	(59)	(59)	(56)	(50)	46·0	39·0
1723	34·0	40·0	45·5	48·0	53·0	59·0	59·5	60·0	56·0	52·0	45·5	42·5
1724	42·0	39·5	40·5	45·0	52·5	59·5	59·0	61·2	58·0	47·5	41·5	38·0
1725	40·0	38·0	41·0	46·5	51·5	54·0	56·8	56·0	55·0	49·0	44·5	39·0
1726	34·0	39·5	39·5	47·2	56·2	61·5	60·8	60·0	58·5	50·3	43·0	35·2
1727	39·5	41·0	41·2	48·5	56·5	58·8	62·5	62·5	58·0	51·5	40·5	38·5
1728	39·0	36·3	44·7	46·9	54·5	61·5	62·5	60·8	55·1	48·4	45·0	34·8
1729	34·2	36·1	37·0	44·8	50·6	59·2	62·3	60·2	61·8	50·1	46·6	41·0
1730	39·3	40·4	43·1	47·7	54·3	57·2	59·5	61·4	59·5	51·6	48·5	38·1
1731	35·5	35·9	42·8	44·2	53·7	60·0	61·3	62·0	59·5	54·2	46·0	41·4
1732	36·3	43·5	43·0	48·0	52·6	58·3	60·8	61·9	58·1	51·7	43·3	36·0
1733	44·5	42·8	42·7	50·0	52·1	59·3	65·0	61·0	55·0	48·3	43·7	45·7
1734	39·7	43·5	46·6	48·7	52·0	57·3	61·1	61·1	56·0	47·1	43·1	39·2
1735	40·0	39·2	42·4	48·0	51·7	56·0	58·6	61·1	57·6	50·5	43·4	41·8
1736	43·5	37·5	44·5	47·5	51·0	60·3	61·5	64·0	58·0	50·8	44·4	43·5
1737	43·2	39·6	43·0	47·8	54·5	60·7	63·3	56·8	57·5	48·1	43·0	40·9
1738	40·2	40·3	41·9	49·9	52·5	57·5	61·5	60·8	54·5	50·3	43·4	43·0
1739	39·2	44·2	42·5	44·0	52·8	59·3	60·8	58·5	55·5	49·3	38·6	37·7
1740	27·0	29·2	39·0	43·5	47·5	55·0	59·5	58·5	57·2	41·5	38·0	36·0
1741	35·0	40·0	39·5	44·7	48·7	59·3	60·0	62·0	58·5	51·8	46·0	39·0
1742	35·5	38·5	39·4	43·8	51·0	59·0	60·5	60·5	54·0	48·5	40·0	34·0
1743	38·5	41·7	41·6	41·7	56·0	60·0	58·8	62·5	57·5	48·0	48·8	40·8
1744	34·5	37·2	40·7	44·0	51·5	58·0	61·5	59·8	55·0	49·0	44·5	38·3
1745	38·8	36·2	40·0	45·5	52·5	54·0	61·0	59·0	57·5	50·5	42·5	36·8
1746	36·5	34·5	37·5	44·5	55·0	57·7	60·5	60·6	57·5	46·0	38·0	41·5
1747	38·0	42·5	36·5	46·5	54·0	58·5	62·5	65·0	58·0	49·0	44·5	41·5
1748	36·5	35·3	35·3	43·3	50·8	58·7	59·8	60·5	57·5	48·5	44·8	42·8
1749	41·6	38·5	41·6	44·2	54·2	53·4	63·0	60·0	56·8	50·2	44·0	40·5
1750	39·2	44·0	46·8	45·8	51·3	57·6	63·0	59·0	59·3	48·6	39·2	39·5
1751	39·2	34·7	43·2	44·8	48·8	58·9	59·6	58·2	54·6	47·0	39·8	37·4
1752	37·8	37·5	42·0	44·3	50·5	58·7	60·0	60·2	57·0	51·0	43·7	39·6
1753	36·0	38·5	42·8	45·5	53·8	58·3	59·4	60·2	55·5	50·0	40·3	40·0
1754	38·0	37·0	38·2	44·0	54·0	56·5	58·5	60·2	57·0	50·8	42·3	38·5
1755	36·0	34·2	39·0	50·0	49·0	60·3	59·0	58·2	56·3	47·2	40·5	39·0
1756	40·0	40·2	42·8	44·0	48·3	56·8	61·0	58·4	56·5	49·0	39·0	37·3
1757	32·5	39·2	40·8	46·5	51·3	57·2	65·1	59·4	56·0	46·7	44·8	37·8
1758	36·7	38·8	41·4	45·0	56·8	58·2	57·5	61·5	53·5	46·5	42·3	39·0
1759	42·6	42·4	43·0	47·5	53·7	59·0	64·8	61·3	56·3	51·7	41·2	36·5
1760	35·5	38·8	43·8	49·0	53·0	59·3	62·5	60·5	60·3	48·5	42·3	43·0

	J	F	M	A	M	J	J	A	S	O	N	D
1761	41·7	42·5	44·3	49·0	53·5	57·7	60·4	61·5	57·5	49·0	43·2	40·0
1762	40·5	39·2	38·7	50·0	55·3	62·5	64·0	59·5	56·5	46·2	40·3	38·5
1763	30·5	40·8	41·8	48·0	50·3	58·3	59·5	59·5	55·5	47·0	42·5	43·2
1764	38·7	38·8	39·0	45·0	54·0	57·0	61·0	59·3	54·5	48·0	40·0	37·0
1765	40·7	32·7	41·0	45·5	52·5	56·8	60·2	59·5	56·0	48·5	39·0	35·0
1766	33·2	35·0	39·5	46·5	49·5	56·6	60·3	61·8	56·0	48·8	45·0	38·0
1767	32·2	41·7	40·5	45·2	50·0	55·0	58·0	61·0	57·3	48·6	44·5	38·0
1768	33·5	40·7	40·5	46·5	54·0	57·0	60·0	60·8	53·0	48·6	42·0	40·2
1769	36·5	36·9	41·0	46·0	52·3	55·5	61·5	59·0	55·0	46·8	42·2	40·6
1770	38·6	40·3	36·5	41·8	50·0	55·5	59·5	60·5	57·0	48·0	41·5	38·5
1771	33·8	37·7	37·6	41·9	54·0	57·7	60·3	57·8	53·9	48·6	43·3	42·0
1772	34·2	35·5	40·0	43·5	50·1	60·9	62·5	61·0	55·4	53·1	44·9	40·6
1773	39·2	36·6	43·7	46·9	50·5	58·4	60·6	62·9	54·3	49·8	41·6	38·8
1774	33·0	39·7	43·5	47·4	51·7	58·4	60·9	61·0	54·5	50·6	40·7	38·3
1775	40·3	42·9	42·8	49·7	54·6	61·8	62·1	60·5	57·8	48·8	40·7	40·1
1776	29·2	38·8	43·6	48·9	51·5	57·4	61·4	59·3	55·3	50·4	43·1	40·0
1777	35·4	36·1	44·2	44·9	53·0	56·5	59·6	60·7	58·3	50·3	44·5	36·6
1778	35·5	37·7	40·3	46·7	51·1	59·8	63·2	62·2	54·0	45·4	44·1	43·0
1779	37·2	46·3	46·2	49·0	53·4	57·9	64·2	63·7	59·4	51·6	42·3	37·5
1780	30·3	35·7	46·3	43·4	55·1	57·6	62·2	63·6	60·0	48·4	40·0	37·7
1781	35·8	40·6	43·9	48·6	53·7	61·1	63·3	63·1	57·6	51·1	43·7	41·7
1782	41·4	35·4	39·3	41·4	48·2	58·9	60·0	57·5	56·0	45·6	36·2	37·0
1783	38·1	37·9	38·0	50·2	50·7	58·7	65·8	60·4	55·1	49·6	43·1	36·8
1784	31·0	34·5	36·8	42·3	56·3	56·7	59·3	57·2	58·6	46·0	41·9	32·5
1785	38·2	32·7	34·2	47·1	54·2	60·9	61·0	57·1	56·5	47·7	42·1	37·0
1786	36·8	38·1	35·8	46·6	52·1	61·0	59·0	59·1	53·0	45·5	38·0	37·1
1787	38·5	42·7	44·3	45·4	52·7	57·0	60·4	60·0	55·0	49·7	40·1	38·9
1788	39·1	38·8	38·5	49·0	56·8	59·7	60·5	60·4	56·2	49·6	43·0	31·5
1789	34·7	41·0	35·8	45·3	54·5	57·2	59·8	61·8	55·6	47·5	40·3	43·0
1790	39·8	43·8	43·6	42·9	53·5	58·2	58·8	60·1	53·9	50·5	43·0	39·8
1791	39·8	40·5	43·7	49·2	51·1	58·5	59·5	60·6	56·9	48·0	42·6	34·0
1792	36·2	40·1	42·6	50·0	50·3	55·8	59·5	62·5	53·3	47·9	44·7	39·7
1793	37·1	40·3	39·6	43·1	51·6	56·3	63·6	59·0	53·0	52·4	42·8	41·6
1794	35·2	44·9	44·6	50·3	52·3	60·3	64·6	59·9	54·5	49·2	42·9	38·7
1795	26·5	33·4	39·0	45·9	51·6	55·7	59·3	61·8	60·8	53·0	40·1	43·9
1796	45·2	40·5	39·5	50·4	50·6	57·1	58·4	60·6	58·2	46·6	40·2	31·4
1797	38·3	40·2	39·8	45·4	52·3	56·5	63·2	60·4	54·5	46·8	40·5	40·7
1798	38·5	39·2	41·2	50·8	55·3	62·4	61·3	61·5	56·5	49·9	40·5	34·7
1799	35·0	37·0	38·1	41·7	49·3	57·3	59·4	57·9	55·3	47·0	42·1	34·4
1800	37·1	36·0	39·2	48·7	54·2	57·0	63·8	62·3	57·1	48·6	41·8	38·0
1801	40·3	40·7	44·0	47·0	53·7	58·7	60·9	62·7	57·5	50·4	40·7	34·7
1802	34·9	38·7	42·0	48·0	51·0	56·6	56·3	62·9	56·9	50·2	41·1	38·5
1803	35·2	38·2	43·3	48·3	50·9	56·6	63·7	60·9	52·6	48·8	41·0	39·9
1804	42·5	37·2	40·4	44·4	55·9	60·9	60·7	60·0	57·5	51·2	43·8	35·8
1805	35·8	39·3	43·1	46·9	50·3	55·7	60·8	61·6	58·2	46·7	40·7	38·4
1806	39·6	39·8	41·1	44·2	53·7	58·8	59·8	61·2	56·1	51·0	46·1	44·3
1807	37·0	38·6	37·3	45·9	53·3	57·5	62·8	62·5	50·9	52·5	37·3	35·5
1808	36·7	37·0	37·8	42·5	56·7	58·6	65·1	62·0	54·8	45·0	42·8	36·0
1809	35·6	42·3	42·8	41·3	55·6	56·7	59·2	58·7	54·9	50·4	40·3	39·4
1810	35·9	38·3	40·9	46·8	48·6	58·3	59·4	58·3	57·0	49·7	41·8	38·4
1811	34·1	40·3	44·7	48·0	55·0	57·4	60·9	58·0	56·7	54·2	45·8	37·6
1812	36·6	41·5	38·3	41·9	51·7	55·4	57·6	57·8	55·7	48·8	40·8	35·0
1813	35·4	42·5	44·2	45·7	52·9	56·5	59·0	58·1	54·5	46·6	39·7	37·0
1814	26·7	34·6	37·3	49·2	48·6	53·9	60·8	58·4	55·1	46·5	40·5	39·7
1815	32·6	43·7	45·1	46·5	54·7	57·7	58·9	59·5	56·1	50·5	38·1	36·1

	J	F	M	A	M	J	J	A	S	O	N	D
1816	36·8	35·7	39·1	43·9	49·9	55·0	56·1	57·1	53·2	50·5	39·0	37·6
1817	40·1	43·5	41·9	45·7	47·7	59·1	57·4	56·4	55·7	43·5	48·3	36·5
1818	39·9	36·8	40·1	44·5	52·3	61·5	64·7	59·5	55·9	53·6	49·1	38·4
1819	39·9	39·7	44·3	47·5	52·7	56·1	61·6	63·4	56·1	48·4	39·3	34·6
1820	31·5	37·8	40·4	48·1	52·5	56·5	60·3	58·5	54·1	46·6	42·0	40·5
1821	38·5	35·7	42·3	49·1	48·9	54·2	58·7	61·5	58·9	50·8	47·5	43·5
1822	40·5	43·4	46·1	46·9	54·8	62·7	60·1	59·4	54·3	51·3	46·7	34·9
1823	31·9	37·5	41·0	44·1	53·9	54·1	57·3	57·9	54·5	47·1	44·7	40·7
1824	39·7	40·5	40·3	45·3	51·2	56·2	60·8	59·2	56·7	49·1	44·9	41·1
1825	38·9	39·1	41·0	48·3	52·8	57·3	62·9	61·3	59·1	51·5	41·3	40·3
1826	32·7	43·5	43·3	47·8	52·2	63·1	64·3	63·6	56·5	51·9	39·9	42·5
1827	35·1	33·3	42·7	48·1	53·5	57·6	61·7	58·6	56·7	52·5	44·5	44·4
1828	41·2	41·3	43·9	47·0	54·3	59·7	60·8	59·5	57·7	50·3	45·4	45·3
1829	32·5	39·7	39·7	44·0	54·5	58·9	59·1	57·7	52·3	46·9	40·1	34·5
1830	31·7	35·9	45·9	48·1	53·6	54·9	61·1	56·7	53·4	50·7	44·5	35·3
1831	34·9	40·7	44·9	48·5	52·7	59·7	62·1	62·5	56·7	54·9	42·1	42·5
1832	37·5	38·1	42·5	47·4	51·7	59·3	60·7	59·7	56·5	51·2	42·6	41·4
1833	34·2	42·0	39·1	45·9	59·1	58·2	60·5	57·7	53·7	50·1	43·9	44·5
1834	44·7	42·0	44·7	45·9	55·4	59·7	62·5	61·1	56·9	51·1	44·0	42·1
1835	37·3	42·3	42·5	47·4	52·3	59·0	61·5	62·5	56·1	48·1	43·9	37·5
1836	38·7	38·3	42·4	44·9	52·0	59·5	59·7	58·3	53·0	47·5	41·5	39·3
1837	36·9	40·5	36·1	40·4	49·9	59·9	62·5	60·3	54·5	50·9	41·4	41·5
1838	29·3	32·7	40·9	42·9	50·9	58·0	60·1	59·1	54·9	49·7	40·2	39·2
1839	37·1	39·3	39·5	43·5	50·3	57·7	58·9	58·3	54·3	48·7	45·1	38·6
1840	39·3	38·4	38·9	49·5	52·5	57·3	56·9	60·7	51·9	45·5	42·5	34·3
1841	34·0	36·4	45·5	46·1	54·9	55·3	56·9	58·3	56·2	47·6	41·3	40·0
1842	33·0	39·5	43·5	46·1	52·6	60·0	58·1	62·7	55·7	44·9	41·9	44·9
1843	39·2	35·5	42·1	47·3	50·7	55·0	58·7	59·5	58·0	46·2	42·2	45·3
1844	38·8	34·9	40·4	49·6	51·5	58·5	59·7	56·3	55·9	48·5	42·7	32·7
1845	37·8	33·7	35·6	47·5	49·1	58·9	57·7	56·3	52·6	49·1	44·1	40·3
1846	43·3	43·5	42·9	46·1	54·1	64·7	61·7	61·9	58·5	49·1	44·5	32·9
1847	35·9	36·3	42·1	43·9	54·1	57·1	63·5	59·3	52·7	51·2	46·2	40·7
1848	34·3	42·9	42·7	46·7	57·1	58·1	60·1	56·5	55·1	49·5	42·4	42·1
1849	39·1	42·3	43·0	43·5	53·7	57·1	59·7	60·1	55·9	48·5	43·9	38·1
1850	33·3	43·5	40·5	48·2	50·2	59·7	61·1	58·1	54·1	46·2	45·3	40·3
1851	42·1	40·4	42·5	45·3	50·7	57·8	58·3	59·9	54·9	51·4	37·5	40·7
1852	40·8	40·5	41·3	46·8	51·1	55·7	63·6	60·5	55·2	46·0	46·2	45·8
1853	41·1	33·0	38·2	45·7	51·6	57·7	58·9	58·5	54·1	50·1	41·3	34·3
1854	38·5	39·7	44·1	48·5	50·6	55·7	59·7	59·4	57·9	48·9	40·9	41·1
1855	36·3	28·9	37·9	44·7	47·8	55·9	62·3	60·2	55·7	49·5	41·5	36·3
1856	38·7	41·5	39·9	46·5	48·9	56·3	59·5	62·5	54·5	51·3	40·7	39·9
1857	36·7	39·7	41·5	45·5	52·3	60·5	61·5	63·4	58·1	52·3	44·9	45·2
1858	38·2	35·3	40·9	45·9	51·5	62·3	58·7	60·5	58·5	49·2	39·8	40·7
1859	40·8	42·3	45·1	45·5	53·1	58·7	64·9	61·1	55·3	49·2	40·7	34·9
1860	38·3	35·1	40·5	42·7	52·7	54·2	58·1	56·5	52·1	49·7	40·1	34·7
1861	35·0	40·9	43·3	45·5	50·8	58·7	59·0	60·6	55·6	53·2	39·5	39·1
1862	39·1	41·1	41·9	47·9	54·2	54·9	57·5	58·3	55·4	50·6	37·7	43·7
1863	40·8	42·4	43·7	47·9	51·1	56·4	59·3	59·9	52·9	49·9	45·2	43·3
1864	36·3	36·1	40·5	47·9	54·3	56·5	59·9	57·5	55·9	49·6	41·9	38·6
1865	35·7	36·1	37·3	51·1	54·7	60·0	61·9	59·1	61·3	49·5	44·0	42·5
1866	42·5	39·9	40·6	47·5	50·0	59·9	59·9	58·5	55·1	51·3	44·5	42·9
1867	34·1	44·5	37·5	48·7	52·5	57·3	58·8	61·3	56·5	48·8	40·7	38·1
1868	39·0	43·4	44·3	47·7	56·3	59·9	64·9	62·3	57·8	47·2	40·9	44·9
1869	42·1	45·5	38·9	50·1	49·2	55·7	63·1	59·9	57·9	49·4	42·5	37·3
1870	38·0	37·0	40·4	48·5	53·0	59·3	63·5	60·3	55·2	49·1	40·5	33·0

	J	F	M	A	M	J	J	A	S	O	N	D
1871	32·9	42·9	45·1	47·7	52·3	55·0	59·4	62·9	54·9	49·7	38·1	38·5
1872	41·0	44·5	44·3	46·8	49·5	57·3	62·7	59·5	55·7	47·1	44·6	41·5
1873	41·3	35·3	41·7	45·9	49·9	57·6	61·1	59·7	53·3	47·4	43·3	41·5
1874	41·9	39·1	44·1	49·7	50·0	57·1	63·1	59·1	56·5	50·8	42·1	31·7
1875	43·5	36·1	41·1	47·4	54·1	57·5	58·7	61·0	58·9	48·1	41·7	39·5
1876	37·7	40·6	40·1	46·3	49·3	57·7	62·9	61·7	54·9	52·3	43·0	42·8
1877	41·9	43·1	40·9	44·6	48·3	59·4	58·5	59·3	52·1	48·7	44·8	40·5
1878	40·3	42·0	41·7	48·1	53·3	59·1	61·9	61·2	55·9	50·3	38·3	31·5
1879	30·7	37·5	40·5	42·3	48·1	55·3	56·5	58·1	54·7	48·0	39·4	33·2
1880	33·7	42·5	43·1	46·3	50·7	56·9	59·9	61·6	58·3	44·7	41·7	41·1
1881	29·3	37·7	41·5	45·1	53·3	56·6	61·1	57·1	54·9	45·1	48·0	39·1
1882	41·3	43·0	45·3	47·2	52·7	55·5	59·3	58·8	53·7	49·9	42·3	39·1
1883	40·4	42·6	35·5	46·5	51·1	57·1	58·1	59·5	55·9	49·5	42·5	40·3
1884	43·7	41·5	43·7	44·9	52·3	57·3	61·3	62·9	58·1	49·0	41·5	39·9
1885	37·3	42·5	40·1	45·9	48·1	57·1	61·3	56·5	53·9	45·5	42·7	38·7
1886	35·7	34·7	39·6	45·7	50·6	56·5	60·7	60·5	56·5	52·3	43·8	35·5
1887	36 3	38·8	37·9	43·1	49·0	59·5	63·1	60·2	53·2	44·7	40·0	36·7
1888	37·8	35·3	37·6	43·1	51·2	55·7	56·7	57·3	54·0	46·3	45·9	40·9
1889	38·2	37·3	40·5	44·7	55·2	59·6	59·5	58·5	55·1	47·5	44·5	37·9
1890	42·3	37·5	43·1	44·7	53·1	56·3	58·1	57·3	58·3	48·9	42·3	30·5
1891	34·4	39·1	38·9	43·2	49·1	58·4	59·1	57·3	57·5	48·9	42·1	39·3
1892	36·1	38·5	36·9	45·1	52·8	56·1	57·7	59·3	54·3	44·7	43·5	35·2
1893	36·0	40·5	44·9	50·5	55·5	60·1	61·5	63·3	55·3	49·8	41·3	40·6
1894	38·2	41·1	44·1	49·4	48·5	56·3	60·6	57·5	52·9	48·7	46·2	41·2
1895	32·3	28·7	41·1	46·7	54·4	58·7	59·4	60·5	59·8	44·8	45·5	39·1
1896	40·7	40·2	44·1	48·4	53·5	61·1	61·1	57·7	55·6	44·5	39·8	39·1
1897	34·8	42·5	43·7	44·7	50·0	59·1	61·7	61·2	53·8	49·9	45·7	40·5
1898	43·9	40·6	39·7	47·3	50·3	56·5	59·5	61·7	59·3	52·3	44·9	45·1
1899	40·9	41·1	41·1	46·1	49·8	60·3	63·1	64·1	55·7	47·9	47·3	35·9
1900	39·9	36·6	38·6	46·9	50·5	58·4	63·9	59·1	56·5	49·7	45·2	44·9
1901	38·3	36·1	39·3	47·5	52·7	57·0	64·4	60·1	57·1	49·4	40·7	38·1
1902	40·5	34·7	44·0	45·5	48·0	57·1	58·5	57·7	55·1	49·3	44·3	40·3
1903	39·5	44·7	44·7	43·5	51·9	55·4	59·6	57·7	55·5	50·9	43·5	38·0
1904	39·3	38·2	39·8	47·7	51·6	56·0	62·7	59·1	54·5	49·5	41·4	38·6
1905	38·4	41·3	44·3	45·1	51·5	58·5	62·9	58·5	54·3	44·7	40·8	40·8
1906	41·5	37·6	41·1	45·1	50·9	57·7	60·4	62·1	57·1	51·6	45·2	37·4
1907	38·5	37·0	43·3	45·6	50·9	54·4	57·4	57·7	56·5	49·7	43·7	40·3
1908	36·5	41·5	39·7	42·8	54·3	57·8	60·4	58·3	55·3	53·3	45·3	39·1
1909	38·3	37·3	38·7	47·7	51·8	53·3	58·2	59·7	53·5	50·8	40·7	39·1
1910	38·3	41·1	42·9	45·1	52·1	58·5	57·5	59·3	54·5	51·1	37·7	43·5
1911	38·9	40·7	41·3	45·5	55·2	58·1	64·7	64·7	57·1	48·8	42·9	43·1
1912	38·5	41·8	44·9	47·9	53·7	57·1	60·9	55·3	52·0	46·7	43·3	44·1
1913	40·1	40·6	43·1	46·5	52·5	57·7	58·3	59·3	57·3	51·7	47·1	41·1
1914	38·6	44·3	42·9	49·7	51·5	58·1	60·5	60·9	55·9	50·6	44·2	40·3
1915	39·3	39·8	41·3	46·3	51·5	57·9	58·3	59·5	56·1	48·3	37·0	41·5
1916	45·5	38·9	37·9	46·7	52·9	53·3	59·5	61·5	55·4	51·1	44·2	35·5
1917	34·9	33·7	37·7	41·7	55·0	59·4	60·9	59·5	57·2	45·5	46·1	36·1
1918	38·9	43·7	42·3	44·1	55·4	55·9	59·7	61·0	53·5	48·7	41·9	44·5
1919	37·3	35·4	38·5	44·8	56·3	57·7	57·1	60·3	54·9	45·3	37·9	41·9
1920	41·3	42·8	44·9	46·8	53·3	57·9	57·3	56·5	55·4	50·7	44·3	39·6
1921	45·2	40·7	45·3	46·4	52·7	58·5	65·3	59·7	57·3	55·0	40·2	43·7
1922	38·7	40·0	40·3	41·9	54·9	56·9	56·6	56·4	53·9	46·7	42·6	42·5
1923	42·0	42·0	43·7	45·7	48·5	54·5	63·5	59·4	54·5	49·4	37·9	38·8
1924	40·5	37·9	39·3	44·5	52·9	57·1	59·5	57·4	55·9	50·2	44·7	44·3
1925	41·6	41·4	40·9	45·5	52·9	59·0	62·3	59·8	52·7	50·8	38·5	37·1

	J	F	M	A	M	J	J	A	S	O	N	D
1926	40·2	44·3	43·4	48·8	50·3	56·5	62·7	61·1	57·9	46·5	42·7	39·5
1927	40·2	39·1	45·1	46·3	52·1	54·6	60·7	60·3	54·5	50·9	43·1	35·7
1928	41·3	42·5	43·3	47·3	51·7	55·3	60·9	59·6	55·0	50·2	45·7	38·2
1929	34·4	32·7	43·1	44·2	52·3	55·9	60·8	59·7	59·5	49·2	44·1	42·5
1930	42·0	36·5	41·5	46·9	51·3	59·5	59·3	60·3	56·5	50·9	43·2	39·7
1931	37·7	39·0	39·5	45·9	52·5	57·9	59·5	57·9	52·7	47·9	46·1	41·6
1932	43·3	37·3	40·5	44·4	50·9	57·3	61·0	62·7	55·3	47·8	43·9	42·3
1933	36·0	39·7	45·1	47·9	54·0	60·0	64·1	63·7	58·9	50·1	42·0	34·9
1934	39·4	38·8	40·7	46·4	52·3	58·9	64·8	59·7	58·2	51·1	42·9	46·5
1935	40·1	42·4	43·9	46·7	49·9	59·2	62·7	61·9	56·5	49·1	44·5	37·1
1936	38·7	36·7	44·7	43·3	52·7	58·4	59·6	60·9	57·9	48·9	41·9	41·5
1937	41·3	42·1	38·5	48·5	53·9	57·4	61·0	62·5	56·1	50·7	41·1	37·4
1938	42·3	41·1	48·3	45·6	51·3	57·9	59·4	61·3	56·8	50·9	48·9	39·9
1939	39·5	42·0	42·5	47·8	52·5	57·5	59·9	61·6	57·6	46·7	47·7	37·7
1940	29·5	36·7	42·8	47·7	54·5	61·5	59·1	60·0	55·1	49·3	44·5	38·9
1941	32·9	38·3	41·1	43·6	48·9	59·1	63·2	58·5	58·1	50·8	43·9	42·0
1942	33·7	32·1	41·3	48·5	51·9	58·0	59·9	61·8	56·5	50·8	40·9	44·1
1943	40·8	42·9	43·7	50·9	53·3	58·0	61·5	60·9	55·9	51·1	43·3	38·3
1944	42·4	38·5	41·3	50·3	52·5	56·3	61·7	62·6	54·5	48·8	43·1	38·5
1945	32·7	44·7	46·3	50·1	53·9	58·3	62·1	60·6	57·9	53·5	45·0	40·9
1946	36·9	42·7	41·2	49·9	51·3	55·5	61·3	58·5	57·2	49·6	46·6	37·5
1947	35·9	28·6	38·5	47·4	56·3	59·9	62·6	65·5	58·9	51·1	44·9	41·1
1948	41·7	40·5	46·9	48·2	52·5	56·3	60·5	59·1	56·9	50·1	45·1	42·3
1949	41·9	42·2	41·1	50·0	52·1	59·5	63·4	62·3	61·3	53·1	43·9	42·4
1950	39·5	41·6	45·3	45·7	52·3	61·1	60·7	60·1	55·2	49·3	42·2	34·1
1951	39·1	38·6	39·3	44·3	50·1	57·2	61·3	58·7	57·4	49·0	47·3	41·9
1952	36·9	38·1	43·9	49·3	56·2	57·9	62·3	60·4	51·3	47·8	39·3	37·1

TABLE 5. EXTREME VALUES FOR PURPOSES OF REFERENCE

	J	F	M	A	M	J	J	A	S	O	N	D
Warmest												
	1916	1779	1938	1865	1833	1846	1783	1947	1729	1921	1818	1934
	45·5	46·3	48·3	51·1	59·1	64·7	65·8	65·5	61·8	55·0	49·1	46·5
	1796	1869	1948	1943	1848	1826	1852	1747	1865, 1949	1831	1938	(1710)
	45·2	45·5	46·9	50·9	57·1	63·1	65·6	65·0	61·3	54·9	48·9	(46)
	1921	1794	1750	1798	1758, 1788	1822	1921	1911	1795	1731, 1811	1743	1852
	45·2	44·9	46·8	50·8	56·8	62·7	65·3	64·7	60·8	54·2	48·8	45·8
Coldest												
	1795	1947	1785	1837	(1698)	1909, 1916	1816	1912	1807	1740	1782	1890
	26·5	28·6	34·2	40·4	(47)	53·3	56·1	55·3	50·9	41·5	36·2	30·5
	1814	1895	1748	(1701)	1740	1749	1802	1725	1952	1817	1915	1796
	26·7	28·7	35·3	(40·5)	47·5	53·4	56·3	56·0	51·3	43·5	37·0	31·4
	1740	1855	1883	1809, (1713)	1817	1814, (1705)	1879	1844, 1845	1840, (1703)	1896	1807	1788, 1878
	27·0	28·9	35·5	41·3	47·7	53·9	56·5	56·3	51·9	44·5	37·3	31·5

Warmest year 1949, 51·1°. Coldest year 1740, 44·3°.

TABLE 6 DECADAL MEANS

	J	F	M	A	M	J	J	A	S	O	N	D	Year
1701-10	37·1	38·4	41·1	45·7	51·7	57·4	60·3	61·1	55·4	48·1	43·7	40·1	48·3
1711-20	37·3	39·1	40·8	45·8	51·4	57·5	60·7	59·4	56·0	49·1	42·8	39·0	48·2
1721-30	38·2	38·7	41·3	46·5	52·9	58·5	60·0	60·1	57·5	49·8	44·1	38·5	48·8
1731-40	38·9	39·5	42·8	47·2	52·0	58·4	61·3	60·6	56·9	49·2	42·7	40·5	49·2
1741-50	37·4	38·8	39·9	44·0	52·5	57·6	61·1	60·9	57·1	49·0	43·2	39·5	48·4
1751-60	37·4	38·1	41·7	46·1	51·9	58·3	60·7	59·8	56·3	48·8	41·2	38·8	48·3
1761-70	36·6	38·9	40·3	46·3	52·1	57·2	60·5	60·2	55·8	47·9	42·0	38·9	48·1
1771-80	34·8	38·7	42·8	46·3	52·5	58·6	61·7	61·3	56·3	49·7	42·5	39·5	48·7
1781-90	37·3	38·5	39·0	45·9	53·3	58·9	60·8	59·7	55·7	48·3	41·1	37·5	48·0
1791-1800	36·9	39·0	40·7	47·5	51·9	57·7	61·3	60·7	56·0	48·9	41·8	37·7	48·3
1801-10	37·3	39·0	41·3	45·5	53·0	57·8	60·9	61·1	55·6	49·6	41·6	38·1	48·4
1811-20	35·4	39·6	41·5	46·1	51·8	56·9	59·7	58·7	55·3	48·9	42·3	37·3	47·8
1821-30	36·3	39·0	42·6	46·9	53·0	57·9	60·7	59·5	56·0	50·2	43·9	40·3	48·9
1831-40	37·0	39·4	41·1	45·6	52·7	58·8	60·5	60·0	54·9	49·8	42·7	40·1	48·5
1841-50	37·0	38·9	41·8	46·5	52·8	58·4	59·7	58·9	55·5	48·1	43·5	39·7	48·4
1851-60	39·2	37·6	41·2	45·7	51·0	57·5	60·7	60·3	55·6	49·8	41·4	39·4	48·3
1861-70	38·3	40·7	40·8	48·3	52·6	57·9	60·8	59·8	56·4	49·9	41·7	40·3	49·0
1871-80	38·5	40·4	42·3	46·5	50·5	57·3	60·5	60·4	55·5	48·7	41·7	38·2	48·3
1881-90	38·2	39·1	40·5	45·1	51·7	57·1	59·9	58·7	55·4	47·9	43·3	37·9	47·9
1891-1900	37·7	38·9	41·3	46·8	51·4	58·5	60·8	60·2	56·1	48·1	44·1	40·1	48·7
1901-10	38·9	38·9	41·8	45·6	51·6	56·6	60·2	59·0	55·3	50·0	42·3	39·5	48·3
1911-20	39·3	40·2	41·5	46·0	53·7	57·3	59·7	59·9	55·5	48·7	42·9	40·8	48·8
1921-30	40·6	39·7	42·6	45·7	51·9	56·8	61·2	59·3	55·8	50·0	42·3	40·2	48·8
1931-40	38·8	39·6	42·7	46·4	52·5	58·6	61·1	61·2	56·5	49·3	44·3	39·8	49·2
1941-50	37·8	39·2	42·7	48·5	52·5	58·2	61·7	61·0	57·2	50·8	43·9	40·1	49·5
MEANS													
1701-1950	37·7	39·1	41·4	46·3	52·2	57·8	60·7	60·1	56·0	49·1	42·7	39·3	48·5
1901-1930	39·6	39·6	42·0	45·8	52·4	56·9	60·4	59·4	55·5	49·6	42·5	40·2	48·7
1931-1950	38·3	39·4	42·7	47·5	52·5	58·4	61·4	61·1	56·9	50·1	44·1	39·9	49·4

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Annual recurrences in Edinburgh temperature

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Summary

Mean daily temperatures at Edinburgh for a period of 150 years are examined for evidence of persistent annual recurrences of unseasonal character. The recurrences are found to be very largely casual in their incidence but, as a result of tests applied, it is concluded that there is only a very small probability that their nature is completely random. Recurrences on the days of ' Buchan spells ' are found not to depart to a significant degree from chance expectations.

1. Introduction

A problem that has interested meteorologists for many years is the reality or otherwise of non-random annual recurrences of periods of unseasonal warmth or cold. Forbes (1860), in a paper dealing with the climate of Edinburgh as revealed by 40 years' observations, discussed the ' periodic anomalies ' shown by the annual temperature curve, as compared with ' standard ' values. He considered that even after a period of 40 years ' the casual fluctuations of daily temperature are far from being eliminated,' but considered also that some of the anomalies might have a real existence. Alexander Buchan (1867) considered the problem in greater detail : in ten years' temperatures (1857-1866) at five well-distributed stations in Scotland he obtained evidence of six cold and three warm periods, which he thought sufficiently well supported by the mean temperatures of the 40-year period previously discussed by Forbes to warrant their being considered permanent features of the Edinburgh climate.

The well-known Buchan spells were subjected to critical analysis by Brooks and Mirrlees (1930). Utilizing a 59-year period of mean daily temperatures, they found no tendency for any of Buchan's periods to occur regularly in London, and concluded that ' on the whole it seems improbable that there exists in our climate an abiding tendency for any part of the year to be either abnormally warm or abnormally cold for the season.' A similar but even more definite conclusion had previously been reached by Marvin (1919), who made a comprehensive study of the problem as it affected the U.S.A.:

> The annual march of daily mean temperatures is a smooth curve devoid of secondary maxima and minima or of perceptible points of inflection. Marked irregularities . . . . do not persist nor have a real existence.

On the other hand Lamb (1950) in an investigation of the annual trend of weather types in the British Isles, has obtained independent evidence for all but one of Buchan's spells, while Flohn (1951) has found all the Buchan cold periods reproduced by the Ben Nevis mean daily temperatures for a period of 20 years. It appeared to the present writer that the question of the reality of annual singularities in British temperatures remains sufficiently open to warrant further investigation, and, in view of the particular association of the Buchan spells with Edinburgh, it seemed appropriate to choose the long record of Edinburgh temperatures for examination. The controversial nature of the problem made it certain that results of practical importance could not be expected, but it is of interest to determine whether or not we are to regard the occurrences of non-seasonal temperature fluctuations as entirely random.

2. Data

Edinburgh temperatures recorded at Blackford Hill (441 ft) from 1901-1950 provided the main material for study. Approximate mean temperatures for each day of this period (' daily temperatures ') were calculated as the means of the individual maxima and minima. From these values mean temperatures for each date (' mean daily temperatures ') were obtained for the 50-year period and for the two 25-year periods comprising it. Use was also made of the mean daily temperatures for previous periods of 50 and 100 years, published by Mossman (1896). The temperatures for these earlier periods were recorded at various Edinburgh sites and at various times of the day, but were adjusted by Mossman to values appropriate to means of maxima and minima at a height of 250 ft. These small variations of site and recording procedure are of no importance in the problem under investigation.

3. Seasonal values of daily temperature

The method of analysis adopted in this paper follows, in its main features, that used by Brooks and Mirrlees (1930) and consists essentially of comparing, for various periods, assumed ' seasonal ' values of mean daily temperature with actual values. The seasonal values adopted were those calculated by harmonic analysis of the monthly mean temperatures, only the first two harmonics being employed in the calculations. Values obtained by smooth graphical interpolation would naturally have fitted exactly the monthly mean values – which those calculated from the first two harmonics do not – but this simpler graphical method was not adopted, because it was thought advisable to avoid any procedure of a subjective nature and also because the small magnitudes and irregular phases of the harmonics higher than the second (see Table 1) suggest that the inability of the first two harmonics to fit precisely the twelve monthly mean values is at least largely due to accidental influences which would disappear over a sufficiently long period.

The correction required for the influence of the unequal length of calendar months in the harmonic analysis process is negligible in most investigations, but it was considered advisable to allow for it in this instance. This was done by ' amending ' the mean values of the 12 calendar months to values appropriate to 12 ' months,' each of length $30\frac{5}{12}$ days, the first with mid-point at midday on 16 January. The amplitudes of the harmonics required the usual correction for the influence of the use of smoothed (monthly mean) values, and this was applied throughout. The results of the complete harmonic analysis of the monthly mean temperatures for the three independent 50-year periods and for the combined period of 150 years are shown in Table 1. The period designated M_1 is a non-continuous 50-year period from 1770 to 1840, for which details are given by Mossman (1896, p. 703). M_2 is a later broken period of 50 years, ending in 1894, the monthly mean temperatures (and also the daily means used later) being calculated from corresponding values for M_1 and for the whole period of 100 years listed by Mossman (1896, p. 705). The period of 150 years is the Mossman period of 100 years plus the period 1901-1950.

TABLE 1. Amplitudes (A) and phases (ϕ) in the Fourier series $A_0 + A_1 \sin(t + \phi_1) + \text{———} + A_6 \sin(6t + \phi_6)$, obtained by analysis of monthly mean Edinburgh temperatures for various periods. ϕ is reckoned from midday on 16 January.

Period	A_0	A_1	ϕ_1	A_2	ϕ_2	A_3	ϕ_3	A_4	ϕ_4	A_5	ϕ_5	A_6	ϕ_6
M_1	47·06	11·13	263°36′	0·69	49°54′	0·26	279°51′	0·33	272°2′	0·20	315°0′	0·24	270°
M_2	47·17	10·46	264°6′	1·29	54°38′	0·24	71°34′	0·19	219°48′	0·30	335°46′	0·20	270°
1901-50	47·15	9·85	260°13′	1·28	55°40′	0·11	66°2′	0·31	149°25′	0·14	163°18′	0·11	270°
150 years	47·13	10·50	262°42′	1·12	54°31′	0·08	16°3′	0·13	217°52′	0·13	299°3′	0·17	270°

4. Method

An answer to the problem under investigation has been sought in three ways : (a) the magnitudes of the mean departures, in the various periods, of the daily temperatures from the corresponding seasonal values are compared with theoretical values based on no tendency for annual recurrences; (b) the method of correlation is used to measure the degree of similarity of these various mean departures; (c) for one of the 50-year periods, the frequencies of occurrence of positive and negative departures on the individual days are compared with chance frequencies.

5. Size of mean departures

For comparison with chance expectancies, the size of mean departures was related to their dispersion. The standard deviation (σ) of the departures of the individual days in the period 1901-1950 was first calculated : σ has a well-marked annual variation and was determined for each month separately. The values obtained are shown in Table 2.

TABLE 2. Standard deviation (σ) of daily departures of Edinburgh temperatures, 1901-1950, from corresponding seasonal values. Units are °F.

J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
5·45	5·15	5·11	4·81	4·63	4·55	3·90	3·66	4·26	4·65	5·14	5·24

The standard deviations listed in Table 2 have standard errors (derived on the assumptions that the distribution is normal and that only 1 in 5 of the successive daily departures used in calculating σ can be counted independent) ranging from 0·22°F in January to 0·15°F in August.

The standard deviation of the mean departures of daily temperature over a period of n years (σ_n) is given by $\sigma n^{-\frac{1}{2}}$, provided the n values are independent. If the n values are not independent by reason of a non-random tendency for recurrence of positive or negative departures on certain dates, σ_n exceeds $\sigma n^{-\frac{1}{2}}$ by an amount which increases with the degree of interdependence. The independence of the temperature departures on the same dates in different years can therefore be judged by comparing the distribution of the mean departures relative to σ_n, assumed as $\sigma n^{-\frac{1}{2}}$, with the normal frequency distribution, in which of course independence is assumed. A point which must be considered is the tendency shown by the successive daily temperature departures, and therefore also by the period means, for high values to follow high and low to follow low, a tendency to which various names have been given by different authors and which is here termed ' coherence ' (Walker 1946). The validity of comparing the distribution of a coherent series with that produced by the accumulation of random errors was investigated experimentally, as follows. The degree of coherence found in the period daily mean departures (correlation coefficient between successive values, $r_1 = +0{\cdot}70$) was applied to a series of 365 random numbers with normal distribution. The frequency distribution of this ' coherent random ' series (Walker 1946) with respect to the sample value of its standard deviation was determined and is listed in Table 3, together with that of the corresponding strictly random series; theoretical values appropriate to the normal frequency distribution are shown for comparison. The distributions, in the various periods, of the mean daily temperature departures of either sign with respect to the corresponding interpolated standard deviations (σ_n) are also listed in Table 3. Attention is confined throughout to the frequency of occurrence of values $\geqslant$ twice the standard deviation ($|x/\sigma_n| \geqslant 2{\cdot}0$). The maximum value of $|x/\sigma_n|$ and the probability of a value as high or higher being obtained by chance, are given in each case.

TABLE 3. FREQUENCIES OF OCCURRENCE (IN 365 OCCASIONS) OF VARIOUS VALUES OF $|x/\sigma_n|$; ALSO MAXIMUM VALUE OF $|x/\sigma_n|$ ATTAINED AND CORRESPONDING CHANCE PROBABILITY

	$\geqslant 2{\cdot}0$	$\geqslant 2{\cdot}5$	$\geqslant 3{\cdot}0$	Maximum $\lvert x/\sigma_n \rvert$	(chance probability)
Non-coherent random series	17	4	1	3·1	(0·73)
Coherent random series	15	3	1	3·2	(0·50)
Theoretical	16·8	4·4	0·95	—	—
1901-1925	24	9	4	3·4	(0·25)
1926-1950	29	7	1	3·0	(0·95)
1901-1950	29	8	3	3·4	(0·25)
M_2	26	4	1	3·0	(0·95)
M_1	21	7	4	3·4	(0·25)
150 years	26	10	5	4·2	(0·009)

Table 3 shows that the distribution of $|x/\sigma|$ is not significantly affected by the conversion of the strictly random series into one with coherence, and it is therefore assumed that the distributions of the various coherent series of temperature departures can legitimately be compared with the theoretical random distribution. (The order in which the terms of such coherent series occur is of course non-random). In every period the number of temperature departures of more than twice the standard deviation exceeds the theoretical number, but the differences are not large, and the maximum values of $|x/\sigma_n|$ in all but one of the periods are such as could readily be produced by chance. It is noteworthy, however, that the differences are greatest for the longest period (150 years), and that the maximum value of $|x/\sigma_n|$ in this case ($x/\sigma_n = -4{\cdot}2$ on 16 November) would be produced by chance only about once in 100 trials.

Table 3 gives the frequencies of departures of combined sign. When the signs are considered separately, large departures (i.e., $\geqslant 2\sigma_n$) appear to be significantly more frequently negative than positive. In the three 50-year periods, for instance, the respective frequencies were 42 and 34. (These compare with chance frequencies of about 25 of each sign). This asymmetry suggests first, that the temperature fluctuations are not purely random, and second, that probable anomalies are mainly negative in character.

The temperature departures found in five of the six periods are represented in Figs. 1, 2, 3. The curves were drawn through points showing mean departures, in blocks of 3 days, referred in each case to the middle day. Lines representing departures equal to twice the standard deviation (σ_n') appropriate to these 3-day means are shown in each case, the latter being calculated on the assumption of only random recurrences of temperature departures on each particular date. In the calculation of σ_n' from σ_n it was necessary to take into account the dependent nature of the departures on successive days. It can be shown (Lewis and McIntosh 1952) that in a series of n terms the 'equivalent number of repetitions' as defined by Chapman and Bartels (1940) is given by

$$\epsilon(n) = 1 + 2(1 - 1/n)\, r_1 + 2(1 - 2/n)\, r_2 + \ldots + (2/n)\, r_{n-1} \quad . \quad . \quad (1)$$

where r_1, r_2 etc. are the serial correlation coefficients of terms one apart, two apart and so on. For the mean temperature departures values of $r_1 = +0{\cdot}70$ and $r_2 = +0{\cdot}47$ were found, so that for $n = 3$, Eq. (1) gives $\epsilon(3) = 2{\cdot}24$. The three values from which each 3-day mean is calculated comprise therefore only $3/2{\cdot}24$ independent values and the standard deviation of the 3-day means (σ_n') is thus smaller than that of the corresponding mean daily departures (σ_n) only in the ratio $\sqrt{2{\cdot}24}/\sqrt{3}$.

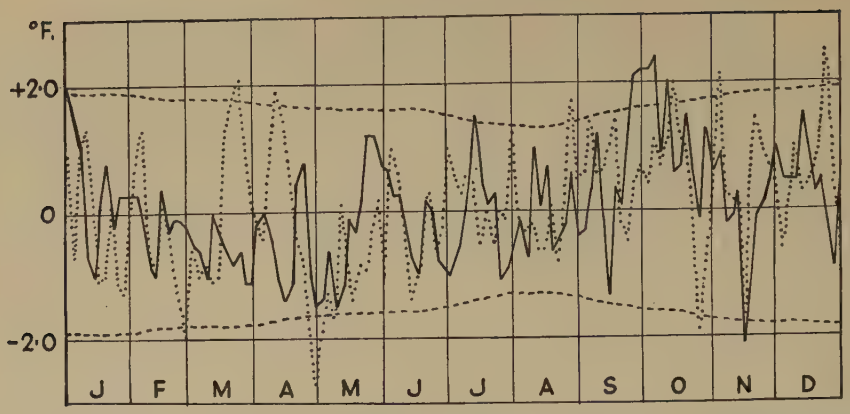

Figure 1. Departures of Edinburgh temperatures (3-day block means) from computed seasonal values, 1901-1925 ——— and 1926-1950 . . . . . Lines representing departures of $\pm 2\sigma_n$ are represented - - - - -.

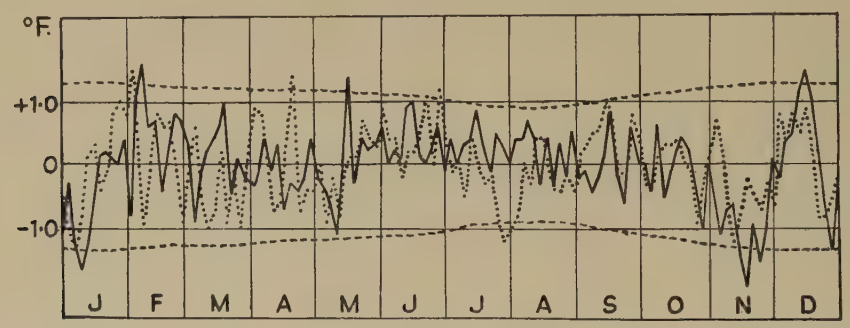

Figure 2. Departures of Edinburgh temperatures (3-day block means) from computed seasonal values in two periods of 50 years, M_1 ——— and M_2 . . . . . .

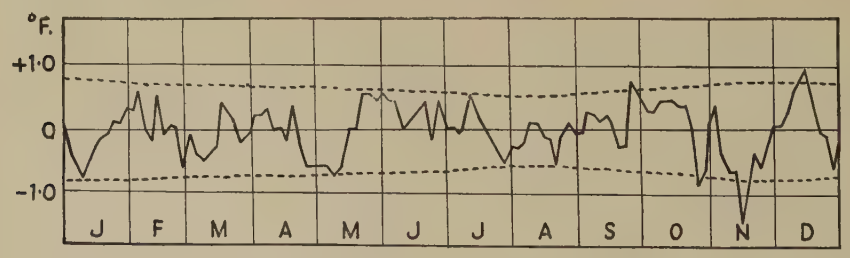

Figure 3. Departures of Edinburgh temperatures (3-day block means) from computed seasonal values in period of 150 years ———.

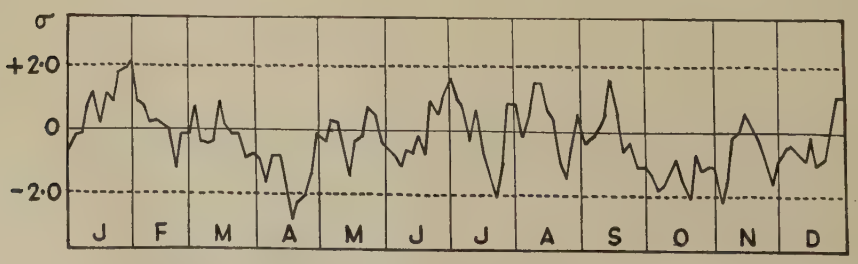

Figure 4. Series of 122 coherent random numbers ($r_1 = +0{\cdot}60$) with normal distribution.

In Figs. 1-3 the vertical scale of temperature departure is the same for all the periods : the decreased size of the departures with increased length of period – evidence that the departures are at least largely random in character – is apparent. The degree of correspondence of the departures in the various periods cannot readily be judged from the diagrams and is considered in Section 6. A coherent random series corresponding to the 3-day block means in Figs. 1-3 is shown for comparison in Fig. 4, and is discussed in Section 9.

6. Correlations of mean departures

Correlation coefficients between corresponding daily mean departures in various periods were calculated and are listed in Table 4, together with the probability of a positive coefficient of the size obtained being equalled or exceeded in samples of 65 independent pairs drawn from two uncorrelated populations. (Calculation from Eq. (1), on making the approximation $r_n = r_1^n$, shows that a series of 365 departures are equivalent to $365/5{\cdot}67 \simeq 65$ *independent* values). The correlation coefficients obtained, though positive in every case, are small, and none reaches a ' significant ' level of probability. Since, however, the first three coefficients of Table 4 are independent of each other, the chance probability of obtaining three positive coefficients of the respective sizes is given by multiplying together the three corresponding probabilities. The result shows that there is only about 1 chance in 1,000 that the mean temperature departures in the various periods are completely independent. This implies that it is highly improbable that the temperature departures from the smooth harmonic seasonal values are purely random in character.

TABLE 4. Correlation coefficients (C) between daily temperature departures in various periods, and corresponding probabilities (P) of chance occurrence of positive coefficients as big, or bigger

Periods correlated	C	P
1901-1925 / 1926-1950	+ 0·17	0·089
1901-1950 / M_a	+ 0·16	0·100
M_1 / M_a	+ 0·14	0·133
1901-1950 / M_1	+ 0·08	0·264

7. Frequencies of positive and negative departures

For the period 1901-1950 the frequencies of occurrence of positive and negative sign of the 50 departures on each date were counted. On most of the dates there were occasions when some of the 50 individual values coincided, to the nearest °F, with the seasonal value, the temperature of the individual days being expressed only to this order of accuracy. For easier comparison with chance results it was necessary to decide on the sign of the departure in each case, and it was therefore assumed that the temperatures on the individual days of coincidence were evenly distributed throughout the interval of 1°F. For instance, the seasonal value for 6 December was calculated as 40·21°F : on 4 occasions in the 50 years the mean temperature on this date was 40°F, and it was assumed that to higher orders of accuracy these 4 values were 39·70, 39·90, 40·10, 40·30, one

being above and three below the seasonal value. The frequency distributions of positive and negative departures on the 365 days are given in Table 5, where, for example, the frequency of 39 of $(\Delta T + 2)$ indicates the occurrence of 2 more positive than negative departures (i.e., 26 positive, 24 negative) on 39 days of the year.

If the sign of a temperature departure on any given date is governed purely by chance, then over a fairly large number (n) of years the frequency distribution of the signs of the departures on the various dates will be approximately given by the terms of the expansion of the binomial distribution $(p + q)^n$, where $p = q = 0{\cdot}5$. This chance distribution of signs is also given in Table 5, the frequencies being rounded off to the nearest whole number : this distribution represents the theoretical chance occurrences of ' heads ' and ' tails ' in 365 trials of tossing 50 pennies.

TABLE 5. FREQUENCIES OF OCCURRENCES OF POSITIVE AND NEGATIVE TEMPERATURE DEPARTURES, 1901-1950, COMPARED WITH CHANCE FREQUENCIES

		0	2	4	6	8	10	12	14	16	18	20	22	24
ΔT	+	29	39	34	35	19	15	12	1	3	0	0	0	0
	−	—	33	37	32	26	21	7	10	6	3	1	2	0
Chance	±	41	39	35	29	22	15	10	6	3	2	1	0	0
Chance	+	41	38	33	26	18	13	7	5	2	2	0	0	0
	−	—	40	37	32	26	18	12	7	4	2	2	0	0

Calculation from the frequencies of occurrence of $\Delta T +$ and $\Delta T -$ in Table 5 shows that negative departures were more frequent than positive in the ratio 9290 : 8960, an excess of 330. There is a probability of only 0·015 that a degree of asymmetry as high as, or higher than this, is the result of chance : it is highly probable that asymmetry of this type is a real feature of the temperature departures, which cannot therefore be expected to be strictly comparable with the symmetrical $\pm$ chance frequencies. In order to make comparison possible, the same degree of asymmetry was applied to these chance frequencies by assuming that the necessary 165 changes of positive into negative sign were distributed evenly throughout the normal frequency distribution. The revised chance distribution, given in the last two rows of Table 5, is closer to the distribution of temperature departures than is the original, and it is apparent that, in the period 1901-1950, on none of the 365 days of the year did temperature departures of one sign exceed those of the opposite sign much more frequently than would be anticipated on chance. However the χ^2 test, measuring the closeness of fit of the two distributions (regrouped according to the signs of their differences, since these appear non-random in the groupings of Table 5), shows that in only about 2 out of 100 trials in random sampling would a fit with the chance distribution be as bad as, or worse than, that obtained with the temperature departures. It is therefore inferred – though without great confidence because of the assumptions made – that there is a fairly high probability that the frequencies of occurrence of positive and negative temperature departures differ significantly, though obviously to only a very small extent, from purely random conditions. Assuming a real tendency for recurrences, Table 5 suggests that these are wholly negative.

8. The Buchan spells

Examination of the 55 ' Buchan spell ' days shows that the cold spells in May and November, and warm spells in July and December, are fairly well supported by the 150-year mean departures, while the other 4 cold and 2 warm spells are not. This does not comprise an independent test, since the observations of 50 of these 150 years were used by Buchan to identify the spells. During the independent period 1901-1950, negative departures exceeded positive by a total of 48 on the 35 cold-spell days, and by 16 on the 20 warm-spell days, while the individual spell best supported was that from 6 to 13 November, when there were 30 more negative than positive departures – an average excess of barely 4 (i.e., 27 negative, 23 positive) on each day of the spell. The spells comprise small coherent samples of days, and these results are not directly comparable with chance expectations, which assume random sampling. Nevertheless it appears certain from these figures that the Buchan spells, either considered as a whole or individually, are far from attaining a level of significance in the period 1901-1950.

In view of the very largely random nature of the temperature fluctuations, there is an obvious danger in accepting spells as real features without careful consideration of the probable range of random fluctuations, and the method used by Buchan may fairly be criticised on these grounds. A point made by Buchan (1867) was that, where mean departures are considered, these should be for short periods only, because of a tendency for anomalies to shift dates slightly in successive periods. (Here again, a danger of confusion with random fluctuations is apparent). The evidence presented in Section 5 does not support any such tendency, since the mean daily departures for the two shortest periods (25 years) are not relatively larger than those for the longest period (150 years). Further evidence concerning this point was sought by using expressions due to Goutereau (1906), who showed that if the series $Y_1, Y_2, \ldots Y_N$ is a set of N normally distributed values, with mean m, arranged in random sequence, then

$$G \equiv \frac{\text{mean variability}}{\text{mean deviation}} \equiv \frac{\Sigma\,|\Delta_i|}{\Sigma\,|x_i|} = 1{\cdot}4142$$

$$\text{with standard error} = 1{\cdot}0684\,(N-1)^{-\frac{1}{2}}$$

$$\text{where} \quad \Delta_i = Y_{i+1} - Y_i, \quad x_i = Y_i - m$$

Using these expressions, it is possible to investigate whether, over a period of N years, there is a significant departure from randomness in the order of occurrence of the departures on any given date – whether, for instance, in the period 1901-1950 there was a significant tendency for runs (of perhaps 5, 10, or 15 years) of relatively high or low temperatures to occur. For this period 1901-1950 the value of G was determined at intervals of 5 days beginning 1 Jan. : values of G on successive days naturally have the same degree of coherence as the temperatures, with only about every fifth value independent. The mean of the 73 values of G thus determined was 1·4000, compared with the theoretical 1·4142. The agreement between these values is well within errors of sampling, since the standard error of the mean of the 73 independent values would be expected to be

$$\{1{\cdot}0684/\sqrt{50-1}\} \times 1/\sqrt{73} = 0{\cdot}0179;$$

also, none of the 73 values of G departs significantly from the theoretical value appropriate to random order of occurrence.

9. Discussion

The analysis detailed in Sections 5 and 6 has shown a probable small non-random element in the occurrence of temperature departures, this element being revealed in Fig. 3 by the ' significant ' departure in mid-November and by the slightly greater than

random number of departures $\geqslant 2\sigma$. A possibility to be considered is that this apparent non-random element may arise from an incomplete determination of the seasonal curve. The true seasonal (i.e., solar-controlled) curve can have only component part(s) integral within the year : the constancy of phase of the first two harmonics is evidence of their ' reality,' while the irregularity of the higher harmonics shows that if any seasonal part remains beyond that given by the first two terms it is small compared with random fluctuations. The large departure in mid-November from the two-term seasonal curve is unlikely to be accounted for by any small additional periodic component, but such a component may perhaps explain the small positive correlations found between the departures in the various periods. Further evidence on this point has been sought by applying Fourier analysis to the departures themselves for the 150-year period and also, for comparison, to the corresponding random series depicted in Fig. 4. The results are given in Table 6.

TABLE 6. AMPLITUDES (A) OF FOURIER COMPONENTS OBTAINED IN ANALYSIS OF MONTHLY MEAN TEMPERATURE DEPARTURES FOR 150-YEAR PERIOD AND OF CORRESPONDING RANDOM SERIES
Units are $x \times 10^{-2}\ \sigma^{-1}$

	A_1	A_2	A_3	A_4	A_5	A_6
Temperature departures	20	22	20	24	26	34
Coherent random series	10	54	25	27	13	17

Table 6 shows that the amplitudes of the third and higher harmonics of the departure series differ quite insignificantly from those of the first two harmonics and are therefore effectively zero. The conclusion drawn from this result is that the probable non-random element is not of a seasonally periodic nature but arises because of a tendency, slightly in excess of chance, for a repetition of unseasonal warmth and cold on certain dates. The large mid-November departure seems not to account for all the discrepancies listed in Table 3 and for the positive correlations found between the various periods, but any remaining parts are so small in relation to random fluctuations as to make their identification impossible.

Finally, so much attention has been given in the course of this paper to a consideration of the small non-random element that it perhaps requires re-emphasis that to a very high degree of approximation the temperature variations conform to purely random fluctuations, as is readily seen by a comparison of Figs. 3 and 4.

10. CONCLUSIONS

The conclusions reached in this paper may be summarized as follows :

(a) The non-seasonal temperature fluctuations are very largely of a random nature, but it is highly improbable that they are entirely so, since (i) the mean daily departures in various periods up to 150 years are larger than would be expected on the assumption that there is no more than chance tendency for recurrences on any date of the year; (ii) the departures in all periods considered are positively correlated; and, less certainly, (iii) the distribution of frequencies of occurrence of positive and negative departures in a period of 50 years departs from the chance distribution to an extent not readily accounted for by sampling. On synoptic grounds we should perhaps expect non-random occurrences of unseasonal warmth or cold to be more likely to consist of large, rather than small,

departures from normal values. If this is so, such occurrences would be more readily determined from the size of mean departures than from the frequencies of occurrence of the respective signs of the departures.

(b) There is strong evidence that any real anomalies are mainly negative in character : the only departures to achieve ' significance ' (at the 5 per cent level or better) in the period of 150 years are those for the 2 days, 16 and 18 November, when the mean temperatures are below normal.

(c) The cold and warm spells detailed by Buchan are not significantly supported, collectively or individually, by the Edinburgh temperature observations for 1901-1950.

(d) The order of occurrence in successive years of the temperatures on individual dates cannot be distinguished from the theoretical random order. This appears to rule out the possibility that there occur significant anomalies which are, in terms of years, short-lived, and which are masked in long-period mean departures by changes of phase.

Acknowledgments

I wish to thank my wife for helping with the calculations and drawing the diagrams. This paper is published with the permission of the Director of the Meteorological Office.

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Climatic fluctuation in the Greenland and Norwegian Seas

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Summary

Decadal means of air and sea temperature for the year, the month of July and for the season December to March were computed from ships' observations for the sea areas between 60°N. and 70°N. between Greenland and Norway. The climatic fluctuations in different parts of the area are compared.

1. Introduction

Much has been written on the subject of climatic changes, including the fluctuation during the last century, and the contributions have increased as the importance has been realized of the marked warming, during the present century, of great regions of the world, particularly the Arctic. Articles by Manley (1944) and Ahlmann (1949) give comprehensive accounts of work in this field. Smed (1947, 1948, 1949), in a series of papers and a summary (1949a), has discussed sea-surface temperature anomalies in the period since 1876 in the ocean regions adjacent to southern Greenland and round Iceland. He has shown that generally there has been a conspicuous rise in sea temperature since the early or middle twenties, and that the rise is generally larger in the more northerly parts of the areas he has considered than in the southern regions. Smed finds also (1950) that in the area 55°-60°N., 10°-30°W. positive anomalies have been predominant since about 1932, but that in the region 50°-55°N., 20°-50°W. there is no apparent predominance of positive anomalies in the period since 1932. He states that higher sea temperatures are caused mainly by increased air temperatures.

Large numbers of punched Hollerith cards for ship observations are now available in some countries. In view of the importance of the present climatic fluctuation it was considered desirable to use these to study the fluctuations of air and sea temperatures in selected parts of the world, including the Arctic, Antarctic and the tropics. This paper will deal with the area between 60° and 70°N., between Greenland and Norway.

2. Criticism of data

The Marine Branch of the Meteorological Office holds about 4 million Hollerith cards for meteorological observations from British ships and 8 million cards which have been duplicated from those for ship observations held in Germany. Only some 160,000 of these are for the area we are concerned with in this paper, i.e., Marsden Squares 251-252 and 217-220 (see Fig. 1), since the amount of merchant shipping trading in this area is not as great as in some other parts of the world.

The distribution of observations as regards position is not uniform, since the majority of observations are along established shipping routes, but as 5-degree squares have been used for most of the computations this is not such a serious disadvantage as might at first be imagined. An even more serious disadvantage is the great variation in the number of observations from month to month (the seasonal variation), and the fact that there are considerably fewer observations in the earlier years of the period considered.

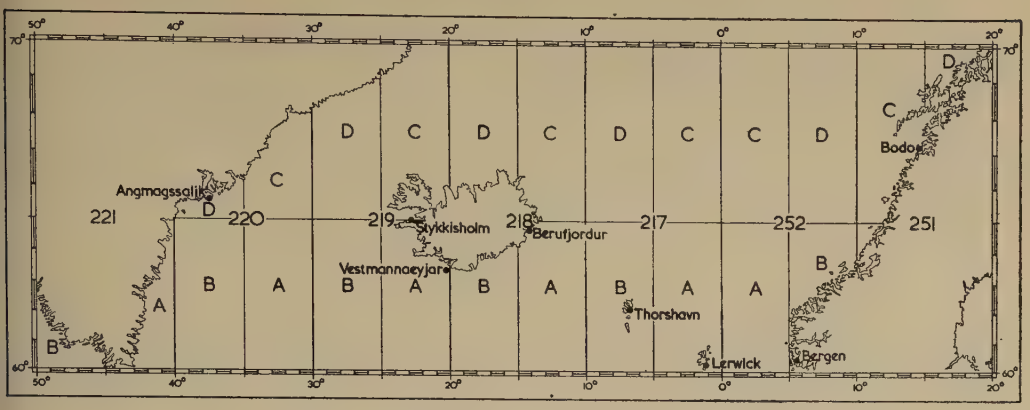

Figure 1. Chart showing Marsden square areas.

The individual observations of air temperature taken aboard merchant ships may occasionally be read with perhaps less accuracy than those from land meteorological stations, but the errors in means which are computed from a large number of observations are likely to be small. On the other hand air temperatures observed at sea do not suffer from many of the defects of exposure which affect those from land stations, and there is no danger of artificial changes in temperature during the period of the observations, such as can occur in land station records if the exposure of the thermometer is altered at some time in the history of the station.

No merchant ship observations are available for the periods of the two world wars and so there are only a few observations from warships during the periods July 1914 to February 1919 and from September 1939 to 1945. Thus it must be remembered when considering temperature means for the decades 1910-1919 and 1940-49 that these are essentially means for the years 1910-1914 and 1945-1949 respectively.

3. Method

The area 60°-70°N. between Greenland and Norway was divided up into 5-degree squares using the Marsden system as shown in Fig. 1. The Hollerith cards for each 5-degree square were sorted into months and then into decades, e.g., 1910-1919, and the means of air and sea-surface temperature in centigrade were computed. The decades chosen were those for the years 0 to 9 rather than 1 to 10 in order to reduce the sorting of the cards. While this choice was a disadvantage in that it prevented direct comparison with results of many previous climatological investigations by various authors, it was felt that in the future most climatological investigations for marine areas, and eventually also most for land areas, will be carried out with the aid of Hollerith machines, which make the use of the decades 0 to 9 a natural choice. Already many countries, including Britain, Germany and the United States, have punched most of their marine data on Hollerith cards.

When dealing with some areas such as the tropics it is hoped to compute the monthly means for each year and then proceed to compute the decadal monthly means : in the region 60°-70°N., however, the number of observations for many individual years during most of the decades are not sufficient to arrive at anything like a reliable value for the mean. The fact that means are often not available for some individual months in many of the years in each decade also prevented the compilation of running decadal means, which would have been advantageous.

The period covered by the observations was too short to use the 30-year means recommended by Hesselberg and Birkeland in 1935.

Where the observations for any particular month for a square are from one ship, as is sometimes the case in the earlier decades, the mean has in many cases been omitted.

Yearly means for each decade have normally been computed only when the whole twelve-monthly means are available. If, however, eleven of the twelve are available for a particular decade together with the whole twelve for the previous or subsequent decade the annual mean for that decade has been computed from the formula

$$\overline{T} = \frac{\Sigma^{11}\,\bar{t}}{12}\,\frac{\Sigma^{12}\,\bar{t}_1}{\Sigma^{11}\,\bar{t}_1}$$

where $\overline{T}$ is the annual mean for the decade, $\Sigma^{11}\,\bar{t}$ and $\Sigma^{11}\,\bar{t}_1$ are the sums of the same eleven monthly means for the decade and the previous or subsequent decade and $\Sigma^{12}\,\bar{t}_1$ is the sum of the twelve monthly means for the same previous or subsequent decade. When an annual mean derived in this manner has been plotted on a diagram, the cross marking the point has been enclosed by a circle.

Where a monthly mean for any decade has been computed from less than 10 observations and has been plotted on a diagram, brackets have been placed around the appropriate cross.

Where possible, decadal annual means have been computed and plotted. In addition, decadal monthly means for July were plotted to represent summer conditions, as means for this month are normally available for longer periods than for other months. Further, decadal means for the season December, January, February and March were computed and plotted. Decadal annual means of air temperature were also computed for the 10-degree squares by meaning the decadal monthly means, which were themselves computed from all observations in the decade in the whole 10-degree square irrespective of position within the square.

For the 5-degree squares between 65° and 70°N., west of 10°E., data are generally insufficient to give enough reliable means for any representation of the results to be worth while. Data are also insufficient in Marsden Square 220 for satisfactory representation.

4. Discussion of results

The decadal annual means of air temperature for the 5-degree and 10-degree squares are shown in Fig. 2. The temperature of 6·3°C for the decade 1940-9 for the whole Marsden square 217 is artificially low owing to an exceptionally high proportion of observations being in the northern half of this square during the decade. The average number of observations used for each of the twelve monthly means of the decade (i.e., the total number used divided by twelve) is shown in Table 1.

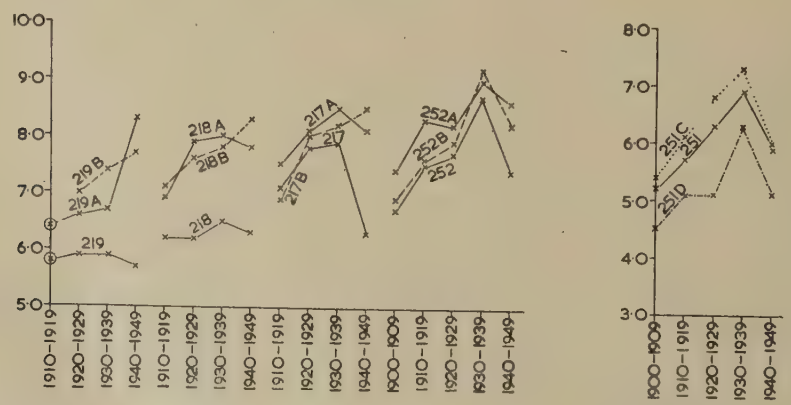

Figure 2. Decadal annual mean air temperatures in degrees centigrade.

TABLE 1. AVERAGE NUMBER OF OBSERVATIONS USED IN COMPUTING MONTHLY MEANS FOR COMPILATION OF VALUES SHOWN IN FIG. 2

Square	1900-9	1910-9	1920-9	1930-9	1940-9
219B			142	186	6
219A		48	378	406	351
218B		37	350	493	106
218A		44	377	572	80
217B		43	371	519	39
217A		45	188	284	50
252A	163	175	88	151	116
252B	194	188	61	47	37
251C	212	209	47	44	69
251D	31	35	22	29	31

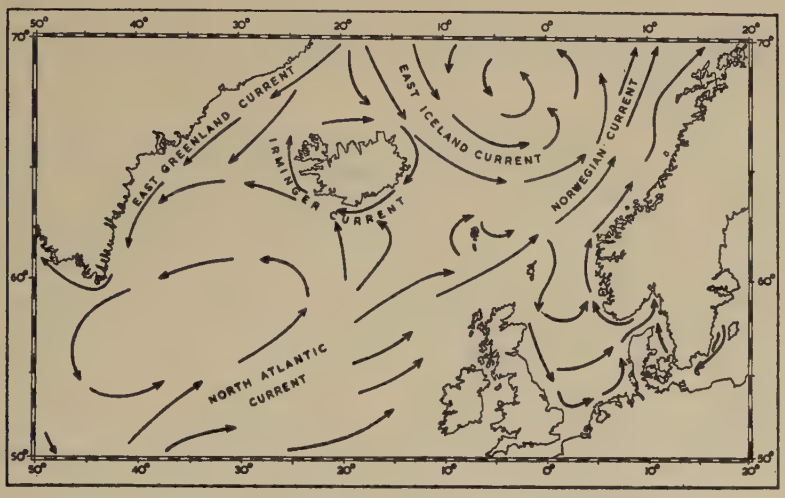

Figure 3. The average surface ocean currents for the whole year.

It will be seen that there has been an almost universal rise in decadal annual air temperature over the sea in the area under consideration until the 1930-9 decade from the earliest time since such means became available, that is normally 1900-9 or 1910-9. This rise in the period 1910-9 to 1930-9 was rather less marked in squares 218B and 219 than in most other parts of the area : it is these squares through which the Irminger Current flows (see Fig. 3).

Between 1930-9 and 1940-9 there has been a marked decrease in temperature in the area east of 5°W. and in square 218A. In squares 218B, 219A and 219B, the area affected by the Irminger Current, the temperature increased between 1930-9 and 1940-9. The temperature also increased in square 217B.

It seems probable that the climatic fluctuation in the region of the Irminger Current in the last few decades is somewhat different from that in those regions in the Atlantic between 60° and 70°N. which are affected by the more direct flow of the North Atlantic Current and its offshoot which passes between the Faroes and the Shetlands and is then known as the Norwegian Current. Now it is generally considered that changes in climate in this area are largely due to changes in the atmospheric circulation and this has been supported by the analysis of atmospheric pressures and winds in the northern Atlantic by Eriksson (1943). Assuming, with him, that the surface ocean currents are largely wind-driven, it is clear that there is a close connection between climatic changes and

changes in the ocean current regime. Eriksson in the same paper considers that the incidence of the maximum increase of temperature in the far north is explained by the fact that the low-pressure area in the north Atlantic has moved abnormally far northwards. As this low-pressure area moved northwards we should expect marked alterations in the orientations of the wind and the ocean-current systems in the area of the Irminger Current in the last few decades : this has doubtless been a major cause of the differences between the climatic fluctuations in this region and those in the east of the area under consideration. The possibility of variation in the Irminger Current over periods of years has been suggested by current observations at Ocean Weather Ship station I at about 60°N. 20°W. It was generally assumed, from infrequent observations made over a long period, that the Irminger Current was such that the surface current at this position would generally be northwards (see Fig. 3). The results of systematic current observations at station I between September 1947 and March 1950, however, showed an eastward or south-eastward predominant current in most months. Unfortunately there are not sufficient wind and current observations available for the sea areas around Iceland north of 60°N. to reach any definite conclusions as to the changes in the atmospheric and current circulations.

The mean values of air temperature for the period 1910-1939 for the whole year were computed, and the anomalies for each decade compiled on the basis of these means are shown in Table 2. The regional distribution of these anomalies of air temperature for the decade 1930-9 is shown in Fig. 4. The fact that there is generally only a gradual transition of anomalies from square to square supports the assumption that the decadal means computed do give a reasonable representation of the actual variation of air temperature. It is likely that a few means, however, may not be truly representative as a rather more uneven distribution than usual with regard to time and space within the square may have caused an appreciable bias in the computations of these few means.

TABLE 2. THE ANOMALIES OF DECADAL ANNUAL MEAN AIR TEMPERATURES IN DEGREES CENTIGRADE

Square	1900-9	1910-9	1920-9	1930-9	1940-9
219A		− 0·2	0·0	+ 0·1	+ 1·7
218B		− 0·4	+ 0·1	+ 0·3	+ 0·8
218A		− 0·7	+ 0·3	+ 0·4	+ 0·2
217B		− 0·8	+ 0·3	+ 0·5	+ 0·8
217A		− 0·5	+ 0·1	+ 0·5	+ 0·1
252A	− 1·1	− 0·2	− 0·3	+ 0·5	+ 0·1
252B	− 1·3	− 0·6	− 0·3	+ 1·0	0·0
251C	− 1·3	− 0·6	+ 0·1	+ 0·6	− 0·7
251D	− 1·0	− 0·4	− 0·4	+ 0·8	− 0·4

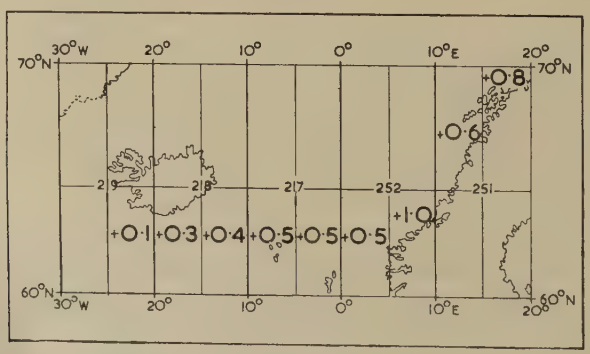

Figure 4. Distribution of anomalies of air temperature in degrees centigrade for the decade 1930-39.

TABLE 3. NUMBER OF OBSERVATIONS USED IN COMPUTING DECADAL MONTHLY MEAN AIR TEMPERATURES FOR JULY

Square	1850-9	1860-9	1870-9	1880-9	1890-9	1900-9	1910-9	1920-9	1930-9	1940-9
219B			21	8	22	28	29	274	275	13
219A			31	13	10	46	107	480	492	311
218B		8	30	14	18	8	43	506	613	202
218A	3	10	17	27	9	7	58	583	678	62
217B		19	53	36	8	16	82	555	738	33
217A		3	104	33	31	41	55	271	448	71
252A			65	14	4	278	298	131	211	95
252B			12	20	6	298	282	94	97	64
251C			58	15	5	259	252	53	49	78
251D			31	5	6	72	70	46	36	7

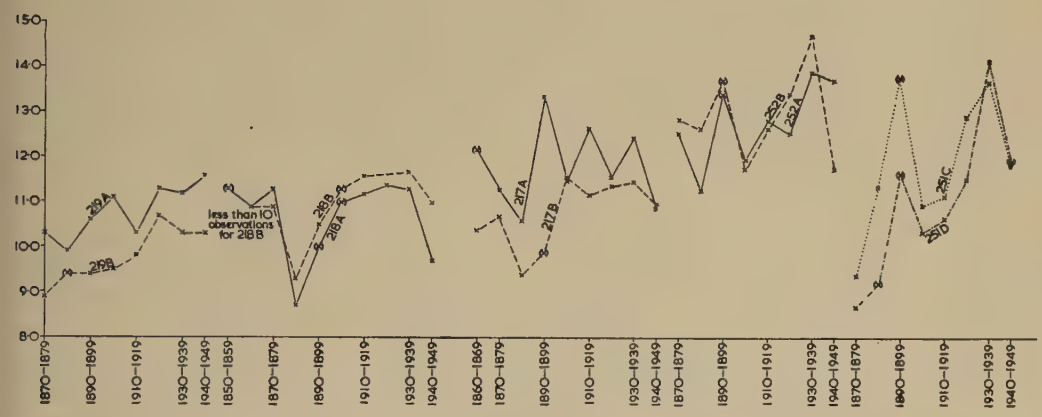

Figure 5. Decadal monthly mean air temperatures in degrees centigrade for July.

The decadal monthly means for July are shown in Fig. 5 and the number of observations on which they are based are shown in Table 3. It can be seen from Table 3 that caution should be used concerning many of the earlier means. The means indicate a general increasing trend from 1890-9 to the present time, but considerable variations from decade to decade. The maximum for the decade 1930-9 is non-existent in the west of the area, but shows up, on moving eastwards, as we reach square 217 and is most marked in square 251. A maximum for the decade 1890-9 exists east of 5°W. but not west of this. The decrease from 1930-9 to 1940-9 is well marked east of 20°W., but does not appear in the means for squares 219A or 219B. The observations available for 1950-1 for squares 218A and 218B show a rise in the July means over those for any of the previous decades. This may mean that the general rising trend in the decadal means of air temperature for July still persists and that the lower temperatures in the years 1945-9 represent a short-period variation in this trend.

The decadal means for the season December to March are shown in Fig. 6 and the number of observations on which they are based are shown in Table 4. The decadal means are the averages of the four decadal monthly means for the relevant months. Observations are not sufficient for square 219 for this season to make any representation of the results worth while. The maximum for the decade 1930-9, very marked in the east of the region, does not show up west of 5°W. In fact in squares 217B, 218A and 218B there is a marked minimum in this decade. Means for each decade of years 0-9 were also computed for various land stations in the area, although the results were not included in the diagrams owing to lack of space. Means so obtained for the season December

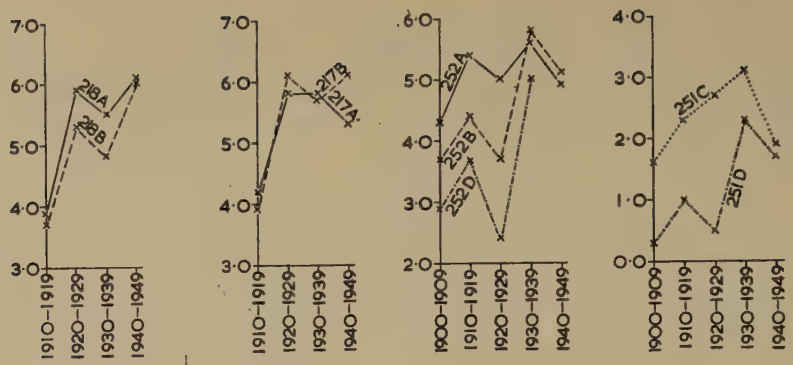

Figure 6. Decadal mean air temperatures in degrees centigrade for the season December to March.

TABLE 4. Average number of observations used in computing monthly means for compilation of values shown in Fig. 6

Square	1900-9	1910-9	1920-9	1930-9	1940-9
218B		11	179	275	75
218A		11	189	355	105
217B		14	254	381	41
217A		14	109	235	37
252A	141	191	65	120	153
252B	155	191	76	49	30
252D	72	81	10	17	
251C	177	211	57	52	67
251D	10	25	25	48	58

to March for Angmagssalik, Stykkisholm and Vestmannaeyjar also show a marked minimum in the decade 1930-9. Again it is clear that the climatic fluctuation in the east of the area is different in character from that in the west, although there is in both regions an overall upward trend in winter temperatures during the present century. Eliminating this overall trend and comparing the results say for squares 218A and 252A for the period 1910-9 and 1940-9 we see that warm winters in the east of the area are associated with cold winters in the west and vice versa. It is interesting to compare these results with those of Walker and Bliss (1932), who show that warm winters in Scandinavia and cold winters in Greenland appear to be associated. The differences in character of the climatic fluctuations in the eastern and western regions of the area considered are doubtless due largely to the fact that the regions are influenced by wind and surface ocean current regimes that are widely different in character. Fig. 3 shows this difference in the ocean current regimes.

The decadal means of sea temperature for the sea areas for the year, for the month of July and for the season December to March, which were computed by the same methods as those for air temperature, are shown in Fig. 7. The numbers of observations used in the computations approximated closely to those used for the appropriate computations of air temperature means. The variations of the decadal annual means of sea temperature are in many respects similar to those for air temperature for the same areas, as might be expected : Smed (1947a) has also found this close similarity between means of air and sea temperature in an investigation of monthly means at Horns Rev Light Vessel during the years 1880-1939. The decrease, however, in the east of the area in the annual means for the decade 1940-9 as compared with those for 1930-9 is not so marked for sea temperatures as for air temperatures. This might suggest that these lower air temperatures

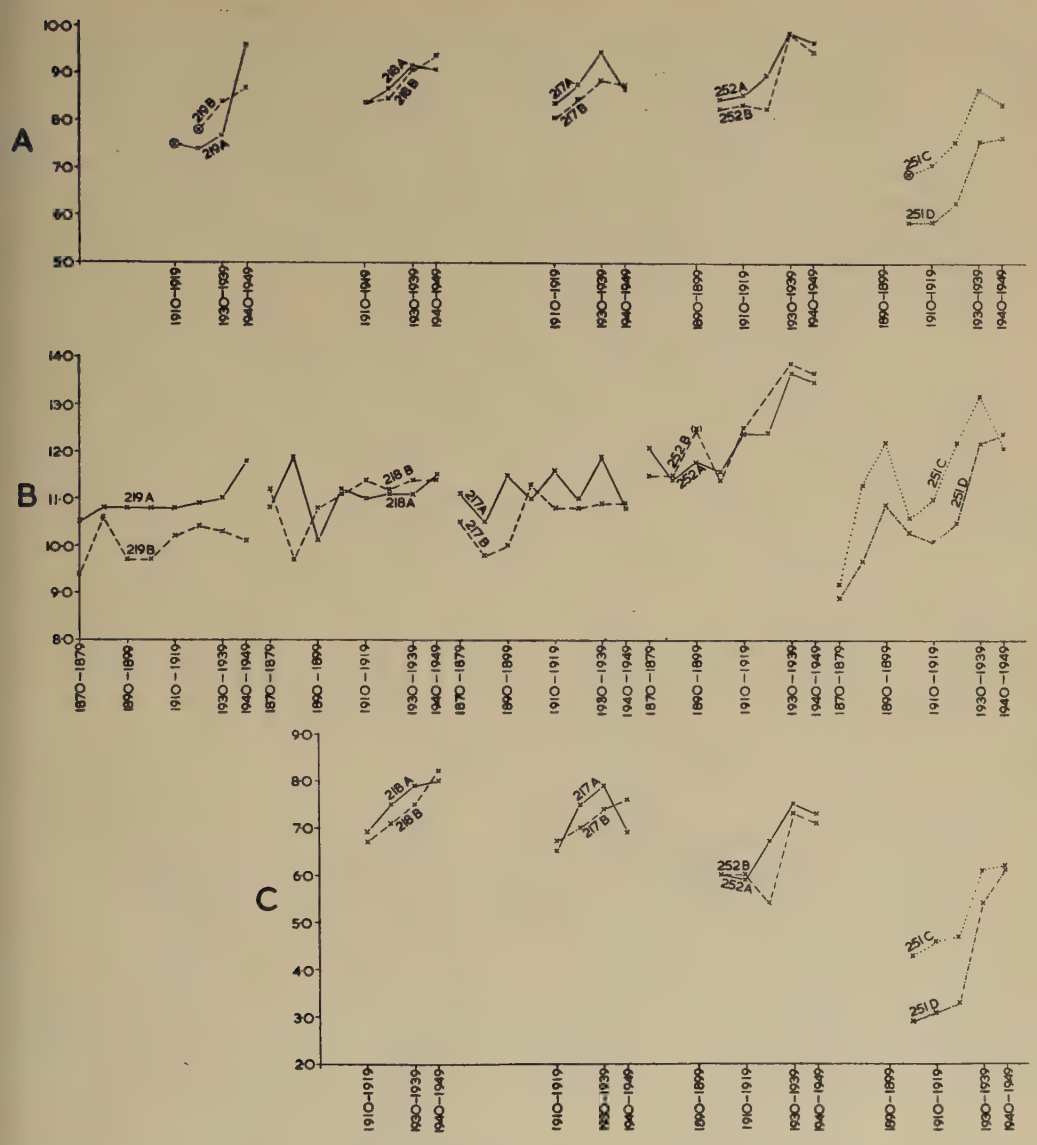

Figure 7. Decadal mean sea temperature in degrees centigrade. A. Annual means. B. Monthly means for July. C. Means for the season December-March.

for the 1940-9 decade were partly due to the abnormally large effect of cold continental air masses or air masses from northerly regions : however, if the cause was due to cold continental air masses one would expect the decrease in air temperature to be greater in winter than in July, but this is not borne out by the results in Figs. 5 and 6. This decrease in decadal monthly means for July from 1930-9 to 1940-9 occurs for air temperatures east of 20°W., but for sea temperatures only east of 5°W., apart from a slight decrease in square 219B.

The decadal means of sea temperature for the season December to March for squares 251C and 251D show an increase of temperature from 1930-9 to 1940-9, whereas those of air temperature show a marked decrease.

The mean values of sea temperature for the period 1910-39 for the whole year were computed, and the anomalies for each decade compiled on the basis of these means are shown in Table 5. The regional distribution of these anomalies of sea temperature for the decade 1910-9 is shown in Fig. 8. The fact that generally there is only a gradual transition of anomalies of sea temperature from square to square supports the assumption that the computed decadal means do give a reasonable representation of the variation of sea temperature, although a few means may not be truly representative.

The increases of annual mean sea temperature from the decade 1910-9 to the decade 1940-9 are shown in Fig. 9.

TABLE 5. ANOMALIES OF DECADAL ANNUAL MEAN SEA TEMPERATURE IN DEGREES CENTIGRADE

Square	1900-9	1910-9	1920-9	1930-9	1940-9
219A		0·0	− 0·1	+ 0·2	+ 2·1
218B		− 0·3	− 0·2	+ 0·4	+ 0·7
218A		− 0·4	− 0·1	+ 0·4	+ 0·3
217B		− 0·4	0·0	+ 0·4	+ 0·3
217A		− 0·5	− 0·1	+ 0·6	− 0·2
252A	− 0·6	− 0·5	− 0·1	+ 0·8	+ 0·6
252B	− 0·6	− 0·5	− 0·6	+ 1·0	+ 0·6
251C	− 0·9	− 0·7	− 0·2	+ 0·9	+ 0·6
251D	− 0·7	− 0·7	− 0·3	+ 1·0	+ 1·1

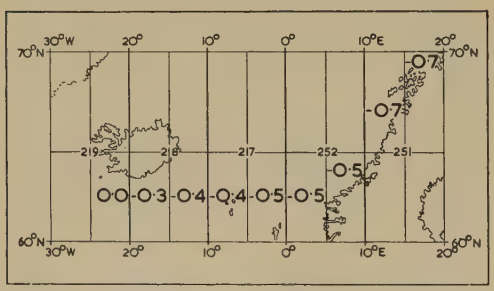

Figure 8. Distribution of anomalies of sea temperature in degrees Centigrade for the decade 1910-19.

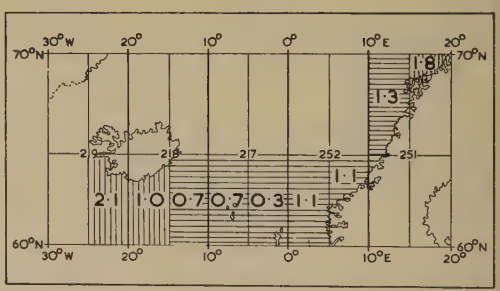

Figure 9. Increase of decadal annual mean sea temperatures in degrees centigrade from decade 1910-9 to decade 1940-9. The vertical shading denotes areas in which mean sea temperature increased from decade 1930-9 to decade 1940-9 and horizontal shading the areas in which temperature decreased.

5. CONCLUSION

The general trend of increased temperatures in northern parts of the Atlantic Ocean during the present century, at least up to the 1930-9 decade, is apparent in the decadal means for the sea area between 60°-70°N. between Greenland and Norway both for air temperature and sea temperature. The character of the fluctuations in the eastern and western parts of this area is, however, very different.

ACKNOWLEDGMENTS

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Equations of transfer for the total flux of terrestrial radiation

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If the present trend of research continues, non-equilibrium problems of atmospheric radiation will have to be solved by means of successive approximations using a computing machine or graphical methods. The only hope of relief from this tedious prospect lies in the development of differential equations involving the total flux of terrestrial radiation. For monochromatic radiation Schwarzschild's equation expresses Kirchoff's and Lambert's radiation laws, which apply to each point in the radiation field, and it might be hoped that a similar, although more complicated, equation might be developed for the total flux of radiation in all frequency ranges. Attempts to do this have been made by Brunt (1929 and 1930) and Malurkar and Ramdas (1932), and some confusion has arisen from the fact that they obtained two entirely different approximate equations. It is difficult to assess these two attempts because the methods used are not analytic, and approximations are made before rather than after finding a general equation of transfer. An analytic approach is however possible, which clearly exposes the impossibility of advance in this direction.

Brunt (1944) gives Schwarzschild's equation for a stratified atmosphere in the form of two equations which, after simple manipulation, yield

$$\frac{\partial^2 \phi_\nu(z)}{\partial z^2} - \frac{1}{k_\nu(z)} \frac{\partial k_\nu(z)}{\partial z} \frac{\partial \phi_\nu(z)}{\partial z} - k_\nu^2(z)\, \phi_\nu(z) = 2k_\nu(z) \frac{\partial B_\nu(z)}{\partial z} \quad . \quad . \quad (1)$$

where ϕ_ν = monochromatic radiation flux at frequency ν,
k_ν = volume absorption coefficient,
z = a space co-ordinate,
$B_\nu(z)$ = the rate of emission of energy from a black body at the temperature of z.

The required equation of transfer must involve the total radiation flux $\phi = \int_0^\infty \phi_\nu \, d\nu$. Eq. (1) can only lead to an exact equation for ϕ for grey radiation, i.e., $k_\nu = k$, when, after integration with respect to ν

$$\frac{\partial^2 \phi(z)}{\partial z^2} - \frac{1}{k(z)} \frac{\partial k(z)}{\partial z} \frac{\partial \phi(z)}{\partial z} - k^2(z)\, \phi(z) = 2k(z) \frac{\partial B(z)}{\partial z} \quad . \quad . \quad (2)$$

If temperature changes are small compared with the absolute temperature then the right-hand side of Eq. (2) is proportional to the temperature gradient, and a tractable linear equation involving the temperature and the flux is obtained. The absorption spectrum of the atmosphere is however far from grey, consisting of hundreds of lines with the absorption coefficient varying by factors of ten or a hundred between them. k_ν and ϕ_ν are strongly correlated in a way which depends upon the state of all levels of the atmosphere so that terms such as $\int_0^\infty k_\nu \phi_\nu \, d\nu$ cannot, even approximately, be expressed as a function of ϕ. There is therefore no generally valid equation of transfer for ϕ in atmospheric work.

Only if certain terms in Eq. (1) can be neglected can an approximate equation of transfer be obtained. Let us first restrict ourselves to an atmosphere of constant absorptivity so that, in all that follows, the second terms on the left-hand side of Eqs. (1) and (2) are zero. If the first term on the left-hand side of Eq. (1) can be neglected in comparison with the third or vice versa, then, after integrating with respect to frequency, the following approximate equations hold

$$\phi(z) = -2\left(\overline{\frac{1}{k_\nu}}\right) \frac{\partial B(z)}{\partial z} \quad . \quad . \quad . \quad . \quad (3)$$

$$\frac{\partial^2 \phi(z)}{\partial z^2} = 2\,\overline{k_\nu}\,\frac{\partial B(z)}{\partial z} \qquad . \quad . \quad . \quad . \quad (4)$$

where

$$\overline{\left(\frac{1}{k_\nu}\right)} = \frac{\int_0^\infty \frac{1}{k_\nu}\frac{\partial B_\nu}{\partial z}\,d\nu}{\int_0^\infty \frac{\partial B_\nu}{\partial z}\,d\nu} \qquad . \quad . \quad . \quad . \quad (5)$$

and

$$\overline{k_\nu} = \frac{\int_0^\infty k_\nu \frac{\partial B_\nu}{\partial z}\,d\nu}{\int_0^\infty \frac{\partial B_\nu}{\partial z}\,d\nu} \qquad . \quad . \quad . \quad . \quad (6)$$

The averages given by Eqs. (5) and (6) are well-defined and, for the range of temperatures occurring in the atmosphere, may be evaluated once and for all. The variables ν and z are separable since, for a range of temperatures small compared with the absolute temperature (θ),

$$\frac{\partial B_\nu}{\partial z} \simeq \frac{\partial \theta}{\partial z}\frac{\partial B_\nu}{\partial \theta}$$

Since $\partial B_\nu/\partial\theta$ is given by Planck's radiation law, it is not correlated with k_ν and the averages do not depend on the physical state of the atmosphere.

Eqs. (4) and (6) are practically the same as the transfer equation and the means of averaging used by Malurkar and Ramdas. (3) is Brunt's equation if the radiation mean-free-path, which he uses, is given by $2\overline{\left(\frac{1}{k_\nu}\right)}$. The quantity which Brunt evaluates is, in fact, more similar to $\frac{2}{\overline{k_\nu}}$. From the work of Cowling (1950) it can be estimated that, owing to the complicated absorption spectrum of water vapour, $\overline{\left(\frac{1}{k_\nu}\right)} \simeq \frac{100}{\overline{k_\nu}}$ for the lowest layers of the atmosphere. Consequently Brunt's values for the ' radiative diffusivity ' should be increased by a factor of about 100 to allow for the incorrect method of averaging.

It is now clear that the approximate equations of Brunt and of Malurkar and Ramdas are the only possible transfer equations for a non-grey atmosphere, and the mathematical conditions governing their validity can be stated precisely. The question now arises as to whether these conditions have any simple physical interpretation which will indicate when each may be employed. It must be emphasized that since the terms neglected in Eqs. (3) and (4) cannot be evaluated in a general case, it will never be possible to cite *sufficient* conditions; *necessary* conditions may however be found by considering special cases. Only for grey absorption does an exact equation of transfer exist from which neglected terms can be evaluated. As a simple special case therefore Eq. (2) has been taken together with a diffusive heat flux given by the product of a conductivity and the temperature gradient. Solutions for both steady and unsteady states have been exhaustively examined for simple physical criteria which will allow either the first or the third term on the left-hand side of Eq. (2) to be neglected. The possibilities of such criteria are limited since they can only involve the conductivity, the absorption coefficient and the various derivatives of the temperature. The required criteria can of course be stated, but, even for these simple equations, they are always found to be of such complexity as to be quite useless in practice, leading to the conclusion that there is no reasonable physical interpretation of the *necessary*, let alone the *sufficient* conditions for the validity of Eqs. (3) and (4).

We must therefore conclude that the only two approximate equations of transfer for the flux of terrestrial radiation cannot in practice be used for lack of simple physical criteria defining the conditions of validity. Eq. (2) remains useful for a qualitative treatment of radiation problems, but apart from this, future advance appears to lie with computational methods only.

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On the physics of dusty atmospheres

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Summary

Measurements of particle size and concentration of atmospheric dust have been made at Alexandria in various degrees of turbidity. Except for ' dust haze,' reasonable agreement is obtained between the measurements and Koschmieder's equation for visibility in terms of optical attenuation. In ' duststorms ' the required attenuation implies a scatter coefficient per particle 8/3 times the cross-sectional area of the particles, while in occasions of ' dust ' the scatter coefficient implied per particle is 4 times the cross-sectional area of the particles. The results are as follows :

Dusty atmosphere	Visibility (m)	Average concentration (cm^{-3})	Average particle diameter (μ)	90% of particles have diameter $\leqslant$ (μ)	Scatter coefficient per particle/cross-sectional area of particles
Dust haze	5,000-6,000	200	0·5	1·3	
Dust	2,000-2,500	330	1·3	2·0	4
Duststorm	750-1,000	550	2·0	3·5	8/3

1. Introduction

In northern Africa whole days of dusty atmosphere are not unusual, and there is a need for studying the physical properties of the air under such conditions, notably with the use of modern apparatus. Moreover, this question has recently been raised by UNESCO in planning the development of improved living conditions in arid and semi-arid zones.

The ' degree of dustiness ' of the air derives from the size and concentration of the suspended particles. These are known to vary greatly with locality and time, notably over the desert areas (El-Fandy 1949) where enormous additions of dust or sand to the surface air may occur locally and temporarily. In cities only relatively small local additions occur, the suspended particles being mainly raised at a distance and drifting with the wind. For such reasons counts over cities are expected to be rather uniform.

In order to minimize further the effects of temporary additions of local dust or sand to the air by winds exceeding the local critical value, and thus to collect normal samples of the dust forming the greater part of the body of a dusty atmosphere, the observations reported here have been made at the Faculty of Science, Alexandria University, which lies on a height about 22 ft above ground level in a locality surrounded by relatively extensive water surfaces.

2. Size and concentration

The World Meteorological Organization has classified dusty atmospheres into dust haze, dust, duststorms and sandstorms (' Standards and recommended practices,' *Meteorological Codes*, Appendix 3, p. 21 ; published in Montreal for implementation on 1 January 1952), visibility deteriorating from class to class as shown in Table 1. Only when visibility decreases to 1,000 m or less may one of the terms duststorm or sandstorm be used, and here only is particle size used in the international definition.

The critical wind for the development of a dusty atmosphere is highly variable, notably from one locality to another, e.g., the finer the dust in a locality the more frequent the occurrence of dusty atmospheres and the smaller the critical wind. Some observations to illustrate this point were made at Burg el-Arab, in the Western Desert of Egypt (Oliver 1945) and are shown in Table 2, which gives the frequency of occurrence of dusty atmospheres of various intensity,

together with the corresponding mean wind and range of visibility, from 1941 to 1945. The effect of the fine dust produced by the mechanised units during the years 1941 and 1942 preceding the Battle of Alamein in decreasing the critical value of the local wind and increasing the frequency of occurrence of dusty atmospheres is clear.

TABLE 1. INTERNATIONAL CLASSIFICATION OF DUSTY ATMOSPHERES

Dusty atmosphere	Visibility (m)	Wind force	Diameter of particles
Dust haze	$> 1{,}000$	Light or calm	Not given
Dust	$> 1{,}000$	$>$ Light	Not given
Duststorm	$\leqslant 1{,}000$	$\geqslant$ Strong	$\leqslant 100\ \mu$
Sandstorm	$\leqslant 1{,}000$	$\geqslant$ Strong	$> 100\ \mu$

TABLE 2. DUSTY ATMOSPHERES AND MEAN WIND; BURG EL-ARAB, W. DESERT OF EGYPT, 1941-1945

Year	Occasions of visibility 700-200 m	Mean wind cm/sec	Occasions of visibility 1,500-700 m	Mean wind cm/sec
1941	31	720	0	—
1942	26	810	0	—
1943	4	1,260	0	—
1944	16	1,210	20	740
1945	2	1,570	3	1,120

Seventeen distinct examples of dusty atmospheres occurred over Alexandria between the beginning of October 1951 and the end of May 1952, dust counts and grading being made with an Owens dust counter and a microscope (oil-immersion objective; magnification 1,600) on each occasion. No sandstorm occurred over the City during the whole period; they are purely local desert phenomena.

As a rule the particles were not spherical but grading within size groups removed any difficulties on this score. Tables 3, 4 and 5 show representative size-distribution counts in each class of dusty atmosphere. Using the usual notations, the average diameter $\bar{d}$ is defined by the equation

$$\bar{d} = \frac{\Sigma(nd)}{\Sigma(n)} \qquad . \quad . \quad . \quad . \quad . \quad (1)$$

and from Tables 3, 4 and 5 it follows that for dust haze $\bar{d} = 0{\cdot}5\ \mu$, for dust $\bar{d} = 1{\cdot}2\ \mu$ and for the duststorm $\bar{d} = 1{\cdot}8\ \mu$. The corresponding means for all the occasions in each class are $0{\cdot}5\ \mu$, $1{\cdot}3\ \mu$ and $2{\cdot}0\ \mu$ respectively. This result, together with the data given in Table 6 below, justify the representativity of Tables 3, 4 and 5.

TABLE 3. DUST HAZE, ALEXANDRIA
4 Feb. 1952, 1100 hr, cloudless sky, wind SW. 10 km/hr, visibility 6 km
($\bar{d} = 0{\cdot}5\ \mu$, concentration $= 170 \pm 15\ \text{cm}^{-3}$)

Size group (μ)	Mean size of group (μ)	n %
$\leqslant 0{\cdot}3$	0·167	50
0·4-0·7	0·484	30
0·8-1·3	0·968	12
1·4-2·0	1·613	5
2·1-3·2	2·580	2
$> 3{\cdot}2$	3·5	1

TABLE 4. DUST, ALEXANDRIA
10 Feb. 1952, 1130 hr, cloudless sky, wind SW. 40 km/hr, visibility 2,000-2,500 m
($\bar{d} = 1{\cdot}2\ \mu$, concentration $= 310 \pm 25\ \text{cm}^{-3}$)

Size group (μ)	Mean size of group (μ)	n %
$\leqslant 1{\cdot}3$	0·645	57
1·4-1·9	1·613	33
2·0—2·6	2·258	5
2·7-3·9	3·225	3·2
4·0-5·2	4·515	1·6
$> 5{\cdot}2$	6·0	0·3

TABLE 5. Duststorm, Alexandria
6 Oct. 1951, 1200 hr, sky obscure, wind southerly 60 km/hr, visibility 800 m
($\bar{d} = 1{\cdot}8\ \mu$, concentration $= 500 \pm 40\ cm^{-3}$)

Size group (μ)	Mean size of group (μ)	n %
$\leqslant 1{\cdot}3$	0·645	34
1·4-1·9	1·613	22·5
2·0-2·6	2·290	19·3
2·7-3·2	2·903	14
3·3-3·9	3·548	6·4
4·0-4·5	4·192	3·2
$>4{\cdot}5$	6·0	0·5

TABLE 6. Further examples of grading (Alexandria)

Dust haze, 31.1.1952. Wind, SW. 5 km/hr. Visibility, 5-6 km.				
Size group (μ)	$\leqslant 0{\cdot}3$	0·4-0·7	0·8-1·3	
n %	59	30	8	
Dust haze, 26.1.1952. Wind, S. 10 km/hr. Visibility, 6 km.				
Size group (μ)	$\leqslant 0{\cdot}3$	0·4-0·7	0·8-1·0	1·1-1·6
n %	47	31	12	6
Dust, 9.3.1952. Wind, W. 30 km/hr. Visibility, 2,000-2,500 m.				
Size group (μ)	$\leqslant 0{\cdot}6$	0·7-1·3	1·4-2·0	
n %	60	25	8	
Dust, 9.2.1952. Wind, WSW. 35 km/hr. Visibility about 2000 m.				
Size group (μ)	$\leqslant 0{\cdot}6$	0·7-1·3	1·4-2·0	
n %	55	27	10	

Measurements of particle size were also made in atmospheres which were not 'dusty' with winds blowing from the desert. The largest particles visible under the microscope ranged from 0·3 μ to 0·7 μ, a few cases of particles of diameters 1·4 μ to 2·0 μ excepted. Farquharson (1937) suggested that the brownish shade nearly always observed at the base of cumulus cloud formed by convection over Khartoum in summer was evidence that even on days of light winds dust existed in the air and was transferred upward.

Further results of grading on occasions of dust haze and dust are given in Table 6, from which, together with Table 3, it becomes evident that within dust haze more than 90 per cent of the particles have diameters $\leqslant 1{\cdot}3\ \mu$. In other words, regarding the dust haze sampled in Alexandria as resulting from dust previously raised at a distance by strong winds, it is seen that about 90 per cent of the particles liable to remain in suspension when surface winds drop to light or calm are only those with diameters $\leqslant 1{\cdot}3\ \mu$. With stronger winds, the greatest number of the particles which drift and give rise to an occasion of dust over a city can be seen to have diameters $\leqslant 2{\cdot}0\ \mu$.

The terminal velocity of fall of small dust particles as given by Cunningham's (1910) equation is shown in Table 7, and it is seen that the particles liable to remain suspended for days are those with diameters less than about 2·0 μ.

TABLE 7. Terminal velocity of fall of dust particles in still air

Diameter (μ)	Terminal velocity (cm/sec)	Terminal velocity (m/day)
0·2	0·0007	0·6
1·0	0·0095	8
2·0	0·0346	30
4·0	0·1380	119
10·0	0·7970	690

It remains to be stated that the visibilities given in the tables are estimates liable to errors within 10 to 20 per cent, that concentrations may vary by up to about 15 per cent from minute to minute on a given occasion and that some discrepancies arise from the combination together of some of the small particles to form bigger particles on the collection slip.

3. Visibility and concentration

The coefficient of attenuation α of light in a dusty atmosphere is given by an equation of the type

$$E_x = E_0 \exp(-\alpha x) \quad . \quad . \quad . \quad . \quad (2)$$

where E_x is the flux density of a beam of light at a distance x which has the flux-density E_0 at $x = 0$, assuming uniform illumination of the atmosphere and homogeneous air conditions. These assumptions are justified with a completely overcast sky or a cloudless sky. Such conditions usually prevail with dusty atmospheres in Egypt, notably with those which arise from desert 'khamsin' depressions.

Koschmieder's formula for the visual range or visibility S in terms of α is

$$S = \frac{3{\cdot}91}{\alpha} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Giving S the successive values 0·7, 1·0, 2·0, 2·5, and 6·0 km, α takes the respective values 5·21, 3·91, 1·96, 1·56 and 0·65 km^{-1}.

Regarding the attenuation of a beam of light in a dusty atmosphere as due to scattering, it is evident that most of the particles of dust which enter in this process have diameters comparable with (or almost equal to) the wavelength of the light scattered. These particles, therefore, have the so-called 'critical size' (Axford, Sawyer and Sugden 1948), and the scatter coefficient per particle under such conditions is given by a complicated function whose value varies between the geometrical cross-sectional area of the particles and four times this area. In the duststorm, for example, with $\bar{d} = 2{\cdot}0\,\mu$, the attenuation coefficient α per km lies between $3{\cdot}14 \times 10^{-3}\,n$ and $1{\cdot}26 \times 10^{-2}\,n$, where n is the equivalent concentration per cm^3, so that from Eq. (3) with $S = 0{\cdot}75$ km, n lies between about 1,600 cm^{-3} and 400 cm$^{-,3}$ while with $S = 1{\cdot}0$ km, n lies between about 1,200 cm^{-3} and 300 cm^{-3}.

Alternatively, using the observed values of visibility (S = 750 m to 1,000 m) and concentration (n = 550 cm^{-3}) in duststorms, with $\bar{d} = 2\,\mu$, the scatter coefficient per particle is found to be about 8/3 times the cross-sectional area of the dust particles. In the same way, it can be shown that in the so-called occasions of dust and in dust haze the corresponding values of the scatter coefficient are 4 and 17 respectively. The value 17, being strange, is mainly due to the discrepancies arising from the combination together of some of the smaller particles to form bigger particles on the collection slip, and to the fact that visibility estimates vary greatly along various directions in haze. They may vary from 6 km in one direction to 10 km in another.

For very small visibilities, say S = 5 m or 0·5 m, of the order met with in severe storms over the desert or in 'haboobs,' Koschmieder's formula gives concentrations of order 10^5 or 10^6. Under such conditions dust becomes an important thermodynamical factor (El-Fandy 1949).

Acknowledgments

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551.558.1

Bubble theory of penetrative convection

By Joanne S. Malkus

I have been studying with considerable interest the paper of this title by Scorer and Ludlam (1953). It appears that some critical comment on this work is direly needed. While the reported experiments on the behaviour of air bubbles ascending through water are highly interesting and suggestive, the claims made by these authors for their paper, namely, (a) that it constitutes a theory and (b) that it sheds light upon cumulus cloud processes must seriously be disputed.

(*a*) *Concerning the applicability of the bubble idea to ' thermals ' in a superadiabatic subcloud layer*

It is at this point, I believe, that the bubble idea is most valuable, provided that it is applied to layers superadiabatic through a significant fraction of their thickness, and that it is not over-generalized. At best, however, the authors have presented only suggestive experiments and fairly vague physical reasoning. A theory usually implies, if not mathematical analysis leading to explicit relations which measurements can test, at least predictions of a qualitative sort. In the case of thermals or bubbles in the subcloud layer, a quantitative framework has already been set up by Bunker (1953; unfortunately his paper was not available in time for use by Scorer and Ludlam). Using the equations of mass, momentum, and energy conservation, and including the effects of loss by diffusion and pressure forces as well as by mixing, Bunker has evolved explicit criteria for the life and penetration of bubbles, depending on their initial size, temperature excess, and the structure of their environment. This work has been checked by simultaneous aeroplane and tower observations, and illustrates the very delicate balance between the bubble structure and that of the environment, especially ambient turbulence. The latter could not, of necessity, have been so much glossed over in the Scorer-Ludlam work had they attempted the evolution and observational checking of quantitative criteria.

Bunker, however, notes the flattening of the bubbles observed in the Scorer-Ludlam experiment and the formation of the larger elements by coagulation of several smaller ones. In summary, I believe that Scorer and Ludlam have contributed valuably to the meteorologists' physical picture of a superadiabatic ground layer, its mechanisms, and the interaction therein of radiation, convection and mechanical stirring.

(*b*) *Applicability of the Scorer-Ludlam idea to cumulus convection*

It is in their attempt to apply their bubble analogy to cumulus cloud processes that these authors wander farthest afield, and must shut their eyes to the accumulating observational evidence. In contradistinction to the bubble model, I should like to contend that a cumulus cloud is more fruitfully regarded as a process or a field of motion rather than an entity or a group of entities. In illustration, it can be demonstrated that a single cumulus may exchange its entire air content with its environment once or even twice during its life cycle.

To deal with specific points : The Scorer-Ludlam model implies that in the growing cloud turret there should be a core in which can be observed air which has risen all the way from cloud base. This core region or ' bubble ' should therefore exhibit a virtual temperature, updraught, and liquid-water content characteristic or very nearly characteristic of the unmixed parcel whose properties may be determined from the adiabatic diagram. Observations indicate that this cannot be the case. In recent aeroplane studies of trade-wind cumulus we were fortunate enough to obtain measurements in several of these active turrets or ' bubbles.' Vertical motion, temperature, moisture, and turbulence cross-sections were obtained. Owing to the response time of the instruments and other factors, the resolving distance for draughts was about 25 m, for temperature and moisture about 100 m. A typical cloud cross-section is shown in Fig. 1, and the run which penetrated the active ' bubble ' core is shown in more detail in Figure 2. The *maximum* virtual temperature observed inside the cloud on this run falls short of that of the unmixed parcel by 2·7°C, while the average differences between cloud virtual temperatures and those of the environment are of the order of a few tenths of a degree (which is enough difference to give rise to buoyancy forces in agreement with observed draughts, entrainment, etc.). In other words, the parcel method gives temperature excesses not merely too large but 600 per cent larger than those observed. The departures in the case of vertical motions are equally striking.

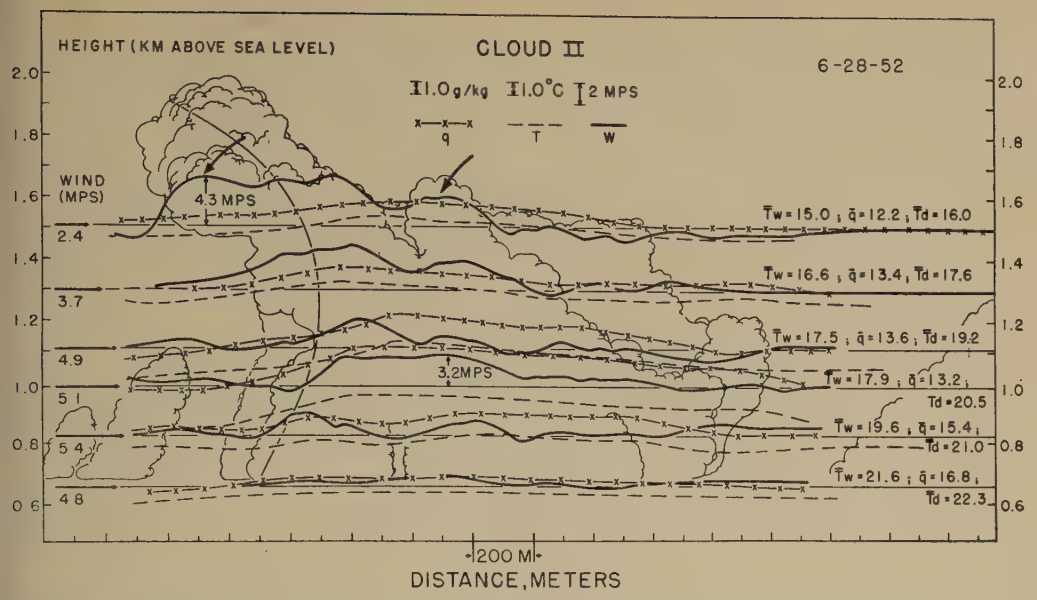

Figure 1. Cross section through actively growing trade-cumulus cloud, over water 30 miles south-west of St. Croix, V. I., 28 June 1952. Reconstructed to scale by aid of photographs. The heavy solid lines are running averages over 150 m distance of updraughts, w. The dashed lines are temperatures, dry-bulb outside the cloud and wet-bulb inside (assuming cloud is saturated). The lines with crosses, q, represent mixing ratios in g/kg. The values of dry-bulb temperature, $\bar{T}_d$; wet-bulb temperature, $\bar{T}_w$, and mixing ratio, $\bar{q}$, at the extreme right are mean clear-air values away from the cloud. These are taken as the reference (origin) values at each level. Arrows indicate ' bubbles.'

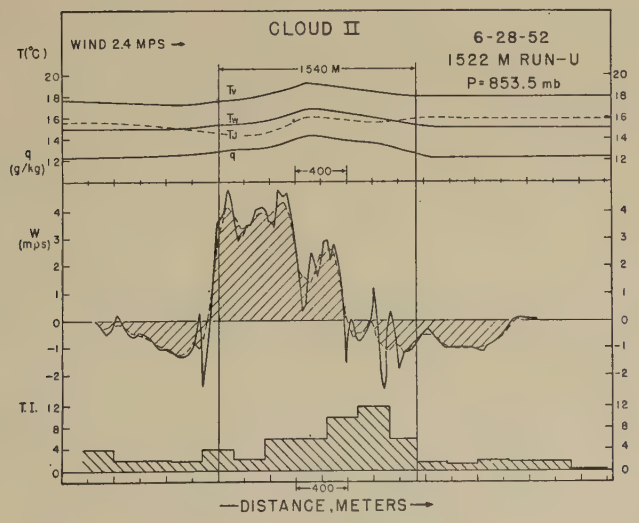

Figure 2. Detailed diagram of first (highest) run through cloud shown in Fig. 1. Run made at 1,522 m elevation; *U* stands for the fact that run was taken flying upwind. Notation same as in Fig. 1, except that shaded curve for draughts are 25-m averages, while the heavy curves are the 150-m averages shown in Fig. 1. *T.I.* stands for ' turbulence index,' a relative measure of the development of small-scale turbulence.

The *maximum* (central core) updraught speed was observed to be 4·5 m/sec ± 0·5 m/sec observational error. A bubble, rising moist-adiabatically from cloud base, would have in its central region a draught larger than 13 m/sec. If such bubbles were present, they must have been reduced to less than 25-m diameter. This, however, is equally incompatible with the Scorer-Ludlam hypothesis since this cloud was picked as an example of the actively growing early stage

and the run in Fig. 2 represents the initial penetration of the cloud by the aircraft. Nor is there any doubt that the central cores of the two turrets (shown by the arrows in Fig. 1) were exactly bisected by the flight. Hence it is not to be anticipated that the maximum liquid-water content, had it been measured, would be even approximately that characteristic of the unmixed parcel. Documentation of these statements and information concerning the observational techniques and instruments used are available at the Woods Hole Oceanographic Institution and will eventually be published together with the finally processed data.

It may be argued that trade-cumulus clouds near Puerto Rico can exhibit a structure quite different from land cumulus over Dunstable, England. This is, of course, possible owing to the probability that different mechanisms bring about the initial condensation in the two localities. A basic structural difference in the clouds is, however, highly unlikely since it is well known that the energy released by condensation in nearly all cumulus far outweighs energy contributed by other causes such as updraughts below condensation level.

There are further contentions by Scorer and Ludlam which must raise serious objections. First that their method of deriving the relative cloud-air horizontal speed gives results similar to those of Malkus (1952a) and that because it appears simpler, their approach is to be preferred. In order to estimate the magnitude of the resistance to cumulus growth, the authors have plotted their observed updraught speeds versus ΔT, the temperature excess characteristic of the unmixed parcel. From this they find an effective resistance coefficient, K, which is then thrown into the equation of motion without any attempt at a mechanistic explanation concerning the physical process or processes opposing the growth of cumuli. Both in using the parcel ΔT and this *ad hoc* assumption of a resistance coefficient K (which one is led by implication to believe is some sort of aerodynamic drag similar to that upon an airfoil), the writers have entirely ignored the basic facts of cumulus-cloud processes and the resistances operating against them. It can be demonstrated by simple order-of-magnitude theorizing (Malkus 1952b; Austin 1948) and has been amply demonstrated by several independent types of observations (Byers and Braham 1949; Stommel 1947; Malkus 1952a) that the most important inhibitory influence upon cumuli is *not in the nature of an aerodynamic drag* (which if operative at all appears to be relatively smaller) but consists of the mixing of the draught air with drier air outside, and thus constitutes an ' eating away ' of the driving mechanism, rather than a simple frictional effect. A pertinent analogy is perhaps found in the automobile which can be caused to stop travelling by drilling a hole in the gas line as well as by applying the brakes. To predict what future course the automobile will take by lumping these two causes of its stopping together in a coefficient is something less than promising. In meteorology, many complicated functional relations could indeed be apparently simplified by combining several parameters together into some kind of a constant to be determined empirically. At best, this procedure does little to clarify mechanisms and to lead to models which predict; at worst, as in the Scorer-Ludlam case, it takes a step backwards and serves to confuse and mislead. In the analyses by Malkus (1949, 1952a) concerning the resistances against cumulus growth, not only was an attempt made to describe mechanisms, but the observed mechanism was introduced quantitatively. Furthermore, the observed relation in the Scorer-Ludlam Fig. 6 between measured updraught speeds and the ΔT for the unmixed parcel is very plausible even though the parcel or bubble model be rejected, since ΔT is dependent only upon the steepness of the environment lapse-rate. When other conditions, such as wind shear, cloud diameter, etc., are roughly equal, the more mechanistic cloud models based on entrainment will also show that the quasi-steady updraughts are proportional to environment instability. The next steps in cumulus studies should be, in contrast to synthesizing parameters and introduction of empirical constants, an attempt to eliminate these by determining a functional form for entrainment rate involving as many variables as may be necessary to construct a time-dependent, life-cycle model. Surely complexity will be inherent in such an attempt; meteorology is a poor area to shun it.

A second considerable objection to Scorer and Ludlam's work is called forth by such diffuse and vaguely written paragraphs as that (p. 103) containing ' fresh bubbles . . . . will be guided . . . . " A physical model predicting the growth of cumulus on the upshear side has been presented by Malkus (1952b). If one accepts the theoretical and observational evidence that in shearing windfields cumulus have a horizontal motion through the air in the upshear direction, the observed partial asymmetry between liquid cloud and updraught is made plausible as being due to an asymmetry in the influx of environment air into the draught. Facilities for direct observational testing of this latter prediction concerning asymmetrical entrainment are now available in Australia, under the direction of Dr. E. G. Bowen, in which pieces of aluminium foil are to be released around the periphery of a cumulus and tracked by radar. If this model

is supported by such tests, as I have little doubt that it will be, the necessity for such metaphysical and anthropomorphic phraseology as the 'guiding' of bubbles may, it is hoped, be replaced by more analytical formulations subject to further specific test by measurement.

In conclusion, this writer is gratified to perceive that, despite their continuing scepticism toward contributions reaching them from this side of the Atlantic, Messrs. Scorer and Ludlam have finally discovered for themselves that mixing between cumulus clouds and the surrounding air is of some non-negligible significance.

Woods Hole Oceanographic Institution,
Woods Hole, Mass., U.S.A.
24 February 1953.

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Reply

By R. S. Scorer and F. H. Ludlam

Convection is a very complicated process and in and under any cumulus cloud there is a whole spectrum of bubble sizes. It is therefore asking too much to hope for a comprehensive dynamical treatment at this stage. The concept of bubbles as described in our paper does, however, lead to many useful conclusions. In Section 6, for instance, it is concluded that bubbles are formed within clouds and that then they are more dilute than if they had ascended without mixing from the base. This is a *prediction*, or consequence, of the theory. Dr. Malkus and others have used the idea of 'entrainment' as a *hypothesis* to explain this dilution, although Dr. Malkus asserts in her own writings that she 'does not purport to answer the very serious questions concerning the mechanisms of entrainment.' Nor does any other paper that has come to our notice.

In treatments of entrainment the outside air is supposed to be instantaneously and uniformly mixed into the updraught at the level at which it is 'entrained' and this is an assumption which is not justified if it can be avoided. Other complications such as 'ambient turbulence' are also arbitrarily introduced. In her treatment to calculate the horizontal velocity relative to the surrounding air, Dr. Malkus uses the entrainment hypothesis together with an estimate of the 'aerodynamic drag.' Even if one accepts that the analogy with a solid body is valid and that the Reynolds number is relevant, which we do not, the values of the drag coefficient which she quotes are entirely experimental and are therefore on a par with her interpretation of our coefficient K.

But there seems to be no good reason for supposing that the forces on an updraught can be divided up in this way. Our theory suggests that they cannot be. Bubbles do reach a limiting velocity and therefore there is some 'drag,' and the best way to discover what it is, is to measure *it* rather than the drag on a solid body in a wind tunnel and to make questionable deductions. Since there appeared to be a roughly linear relationship between upward velocity and buoyancy the coefficient K was thrown up naturally. It simply represents the slope of a line which fits the observations. Its value, which we regard as having been only very roughly indicated, enables the magnitude of the relative horizontal velocity to be calculated as a simple exercise.

Dr. Malkus has no reason to believe that we think of the drag as anything like that on an aerofoil because we state clearly that 'there is no aerodynamic analogy.' Our acceptance of the linear relationship mentioned above and the idea of the shedding of the outer skin of the bubble imply this too. We have observed and performed calculations on the velocity of a bubble, which

is not necessarily or even probably the velocity of the air composing the bubble because of internal circulations. It is the velocity of its leading cap, which is the only clearly defined part of it, such as one sees at the top of a rising cumulus tower, and because such bubbles rise into undisturbed air (there is no evidence of ' ambient turbulence ' in pileus formations) above the tectonic limit they are ideal for measurements. Within clouds, bubbles rise through a confusion of old wakes with a variety of upward and downward motions. From time to time a discrete bubble may be encountered by an aircraft but in the present state of knowledge its rate of ascent cannot be deduced from the updraught measured.

The simplicity of a theory is bound to be one criterion of its value. We are not blind to the complexity of nature, and recognize it in our opening paragraphs. But the bubble idea is not an analogy; it is defined expressly to deal with atmospheric convection, and the experiments in water were only intended to be suggestive. We have frequently been urged to pursue them further but the measurements must be made in the atmosphere for we have no equations to offer us an analogy. The bubble idea lends itself better than the analysis of Malkus and others to dealing with the infinite complications we actually find, and it is our critic's model that is too restricted. Measurements can not readily refute the bubble idea but can help it to take a more quantitative form. Even if the bubbles of the Dunstable clouds were diluted the value of K will not be altered by an order of magnitude, and we have made the useful discovery that the rate of ascent of cloud towers can be related to a sounding. The real issue is not whether Dr. Malkus's half-dozen disposable constants or our disposable form is to be fitted to the observations, but whether the one theory or the other helps us to understand what is actually happening during convection.

Imperial College,
London, S.W.7.
6 March 1953.

551.554

The derivation of the logarithmic velocity profile

Reply

By O. Björgum

The Editor of this *Journal* has courteously sent me a copy of Dr. K. L. Calder's ' Note on the derivation of the logarithmic velocity profile ' (1953).

From the first paragraph of Calder's note one gets the impression that Squire (1948) derived the logarithmic velocity profile in the same way as I did in my note (1952). However, Squire did not show that the shearing stress τ, density ρ and viscosity μ are the only parameters of the problem considered, and therefore he could not claim the solution to be unique. Further, he introduced the concept of eddy viscosity, a complication I have shown to be superfluous.

Calder states in his note that ' Björgum's paper contains a serious misconception in that he considers that in the region of fully turbulent flow near a boundary the velocity should be independent of the kinematic viscosity of the fluid.' I see now that one might get this impression from the fact that I (tentatively) introduced this assumption to begin with. But as it led to the conclusion that U, the mean velocity in the x direction, had to be constant, I replaced it by the new assumption that dU/dz should be independent of the kinematic viscosity, ν (the latter would have been no new assumption if the former had been maintained). Thereby I obtained the logarithmic velocity profile in the forms,

$$\frac{U}{U_*} = C \log \frac{z}{a} = C \log \frac{U_* z/\nu}{U_* a/\nu},$$

$U_* = (\tau/\rho)^{\frac{1}{2}}$, and C and a are constants. Further, as this equation must be a special case of the general functional relation

$$\frac{U}{U_*} = f\left(\frac{U_* z}{\nu}\right)$$

I concluded that C and $U_* a/\nu$ had to be universal constants for the problem under consideration, and then we have

$$\frac{U}{U_*} = C \log \frac{U_* z}{\nu} + \text{constant}$$

It is seen that there is no misconception in this derivation so I can only regret that my failure to state explicitly that the assumption that dU/dz is independent of ν replaced the first one led to Calder (and possibly other readers) not realizing that I was rejecting, as unacceptable, the assumption that U is independent of ν.

On the other hand, the conclusion that U cannot be given by a power law, as stated in my note, is not a necessary consequence of the conditions to be met by U. The conclusion would be valid if U were independent of ν, and I erroneously used the same argument to reject both this independence and the power law.

In his note Calder concludes that dU/dz must be independent of ν from the fact that, in the fully turbulent layer, the shearing stress is equal to the Reynolds stress. This argument needs for its justification a relation between the Reynolds stress and dU/dz which is as yet unknown. Of course, the assumption $\tau = K(dU/dz)$ is always analytically possible, where K denotes the eddy viscosity, but, in order that the argument be rigorous, K then must be assumed or proved independent of ν.

That dU/dz is independent of ν is more easily and quite rigorously seen from the fact that the neglect of molecular agitation in the fully turbulent region immediately implies that $U - U_v$ must be independent of ν, where U_v denotes the velocity at the upper boundary of the viscous layer. From this again it follows immediately by differentiation that dU/dz is independent of ν in the fully turbulent region.

Finally, I should like to express my sincere thanks to Prof. M. Neiburger for valuable criticism in connection with this note.

University of California,
Department of Meteorology,
Los Angeles 24, California, U.S.A.
24 December 1952.

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Critical values of the Richardson number

By J. Clodman

In a recent review that I have made of the literature on turbulence a misinterpretation of some work carried out by Petterssen and Swinbank (1947) was noted. At least two authors (Calder 1949, Sutton 1951) state that the Petterssen and Swinbank investigation led to a critical value of the Richardson number, $\mathrm{Ri}_{\mathrm{crit}}$, of 0·65. Actually this numerical value referred to the ratio of the coefficient of eddy conductivity of heat to the coefficient of eddy viscosity, i.e., $K_H/K_m = 0{\cdot}65$, based on the validity of Eq. (1) below and some other assumptions.

The equation for the gain in eddying energy can be written

$$\frac{\partial E}{\partial t} = K_m \left(\frac{\partial \bar{u}}{\partial z}\right)^2 - \frac{g}{\bar{T}} K_H \left(\frac{\partial \bar{T}}{\partial z} + \Gamma\right) \quad . \quad . \quad . \quad (1)$$

neglecting other terms. The first term on the right represents the gain in eddying energy from the mechanical energy of the mean flow and the second term can be considered the loss due to work against gravity. The Richardson number is defined as,

$$\mathrm{Ri} = \frac{\frac{g}{\overline{T}}\left(\frac{\partial \overline{T}}{\partial z} + \Gamma\right)}{\left(\frac{\partial \overline{u}}{\partial z}\right)^2} \qquad . \quad . \quad . \quad . \quad . \quad (2)$$

If we assume with Richardson that $K_m = K_H$, then there will be a gain in energy if the numerator on the right of Eq. (2) is less than the denominator. This would, of course, give the critical value of Ri as 1. If on the other hand we accept the Petterssen and Swinbank result that $K_H = \frac{2}{3} K_m$ approximately, then there will be a gain in energy if $\frac{2}{3}$ of the numerator is less than the denominator. This implies that the value of $\mathrm{Ri}_{\mathrm{crit}}$ is approximately 1·5 rather than 0·65.

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Meteorological Service of Canada,
315 Bloor Street West,
Toronto, Ontario.
26 January 1953.

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Rainfall at Bidston, 1867-1951

(1) By S. Gregory

In connection with the above paper by G. Reynolds in the January 1953 (vol. **79**) number of the *Quarterly Journal*, a few points require comment. Several of the important values specifically mentioned therein conflict with those obtainable from the *Monthly Weather Report* and from *British Rainfall*. They have presumably been taken directly from the Observatory records, which for some reason differ from those published by the above authorities. Two such differences especially merit comment, in that a worker analysing conditions on the basis of these published records would arrive at conclusions rather different from those of Reynolds.

(i) The annual rainfall for 1933, stated by Reynolds to have been 20·01 in., is given as 19·39 in. in the *Monthly Weather Report* (19·38 in. in *British Rainfall*). The value for 1933 would therefore seem to be some 0·4 in. below the theoretical lower limit of 19·794 in. arrived at by Doodson and Bigelstone. This would seem to invalidate Reynolds's statement that this value of 19·79 in. is ' lower than any observed value, before or since the paper.' This discrepancy was noted by J. Baxendell during the discussion on Doodson and Bigelstone's paper in 1934.

(ii) The heaviest fall on a rainfall day, stated to have been 2·03 in. on 25 June 1891, would seem to have been exceeded, at least according to the *Monthly Weather Report*, on 31 May 1924, with a fall of 2·09 in. (53 mm). Whilst this was measured at 0700 hr, the fall at Hoylake for that same day, and measured at the standard time of 0900 hr, was 3·03 in. (77 mm), which suggests that a fall at least of the order of 2 in. occurred at Bidston during the twenty-four hours from 0900 hr on that day.

On the other hand, analysis of the conditions during the thirty-year period 1920-1949 tends to support the general conclusions set forth by Reynolds, as the following table suggests :

Mean monthly rainfall (in.)	J	F	M	A	M	J	J	A	S	O	N	D	Year
1867-1951	2·31	1·79	1·77	1·65	2·06	2·03	2·70	3·08	2·69	3·22	2·70	2·68	28·68
1920-1949	2·67	1·82	1·48	1·65	2·29	2·04	2·77	2·99	2·46	2·96	2·83	2·47	28·51
Mean daily rainfall (in.)													
1867-1951	0·075	0·063	0·057	0·055	0·066	0·068	0·087	0·100	0·090	0·104	0·090	0·086	
1920-1949	0·086	0·065	0·048	0·055	0·074	0·068	0·089	0·096	0·082	0·095	0·094	0·080	

The validity of the mean annual value thus receives further support, whilst the tendencies in recent decades, as suggested by Reynolds, for (i) March to replace April as the driest month, (ii) August and October to be almost equally wet, but with the highest monthly total falling in August, and (iii) subsidiary minima to occur in June and September, are also emphasized. The tendencies for December, however, to be markedly drier than both November and January, and (on the basis of mean daily values) for November to be virtually as wet as August and October, are features not commented upon in the paper concerned.

Department of Geography, University of Liverpool,
12 Abercromby Square,
Liverpool, 7.
12 February 1953.

(2) By R. S. Read

On p. 147 of his paper G. Reynolds misquotes the official definition of absolute drought which is, in fact (*British Rainfall*) ' a period of at least 15 consecutive days to none of which is credited 0·01 in. of rain or more.' It should be added that any measured quantity between 0·005 and 0·01 in. is credited as 0·01 in., but any quantity less than 0·005 in. is credited as a 'trace.' If the units are mm, a measured quantity lying between 0·05 mm and 0·15 mm is reported and published as 0·1 mm but this amount would not break a period of absolute drought.

Does Mr. Reynolds's vaguer criterion of ' 15 consecutive days without measurable rain ' in fact represent something different from the Meteorological Office definition ?

37 Hartington Road,
London, W.4.
10 February 1953.

The Author replies :

(i) I concur that this discrepancy exists. It would appear that at least from 1929 until 30 April 1935, when the practice was discontinued, there were two rain gauges in use at Bidston. One was emptied twice daily and its readings were utilised for all long-period values, and for the reports regularly sent to the press. This gauge gave a fall of 20·01 in. in 1933.

The other gauge was only used for reports to the Meteorological Office and gave 19·39 in. in 1933. This gauge caught a consistently lower fall than the gauge the readings of which I have analysed and which were considered in the previous papers. Therefore to refer the other gauge's readings to these analysis results is not a valid procedure.

(ii) The fall of 31 May 1924 at Bidston was 2·03 in. according to the manuscript diary for that day, the gauge being emptied at 0900 hr. This figure does not appear in the summary of wet days from which I have extracted my data, and there was no reason to suspect that this summary was incomplete. The source from which the *Monthly Weather Report* received its 53 mm cannot be traced.

(iii) Gregory's last two paragraphs in the main merely reiterate my findings. The tendency for December to be markedly drier than January, as Gregory suggests, is not borne out by the following decadal means of mean daily rainfall (in.) :

	1920-1929	1930-1939
Dec.	0·082	0·086
Jan.	0·084	0·083

The tendency for November to be as wet for August and October is only true when the figures for the three decades are grouped. Taken separately there is too much variation for any such conclusion to be more than a generalization.

I confirm that as stated in my paper, I used the Meteorological Office definition of absolute drought. The phrase ' without measurable rain ' is admittedly different from the *British Rainfall* wording, but I feel that it amounts to the same thing, as the smallest measurable quantity of rain stated in inches is 0·01 in., and all rainfall measurements were quoted in inches in the paper. Possibly Mr. Read interpreted my phrase as ' a period without measurable rain of at least 15 days' duration ' whereas I intended to convey ' 15 consecutive days none of which had any measurable rain.'

Orographic cirrus clouds

By F. H. LUDLAM

(Read 19 November 1952. See *Q.J.*, **78**, p. 554)

Mr. L. C. W. Bonacina : I think that what Mr. Ludlam has found regarding the large vertical effects produced by quite minor features of relief is very important in the general theory of orographic rainfall. In fact orographic effects are beginning to assume so much prominence in meteorology that it seems certain that the skies which we enjoy in the British Isles must be much richer in detail than those which appear over mid-ocean or over the middle of vast continental plains.

I have good reason for asking Mr. Ludlam whether he thinks orographic cirrus can discharge rain. One morning last September about 09 BST when I was walking across an 800-ft ridge in east Devon in a strong west wind with scattered showers there was a sudden light shower with nothing to account for it but a patch of what looked like decayed cirrostratus passing overhead. I could not, however, be sure about the origin of this cloud which may have been a remnant of false cirrus from one of the scattered showers. To windward of the patch there was a wide belt of pure blue sky, so that the shower which fell in quite large drops could hardly have been blown along.

Mr. D. J. Schove : I attempt to associate in temperate latitudes all cirrus, detached false cirrus and persistent contrails with specific fronts. Often the proximity to the frontal surface is necessary to explain, e.g., the persistence of Cb tops, but there are situations in which cirrus is associated with a front but not with a frontal surface. In particular, ' overshoot ' cirrus is characteristically found to the SE· of the typically-orientated warm front within the warm air. Such a situation is shown on Mr. Ludlam's weather map for 29 June 1952 (*Weather*, **7**, p. 304, Fig. 4) and may explain why the cirrus developed in the Midlands rather than west Wales. At the same time, the suggestion that a front was a necessary prerequisite for the formation of the cloud does not conflict with the orographic explanation. Indeed the mechanism suggested by Mr. Ludlam seems to me a very happy example of the combination of theory and observation and I look forward to his future results with great interest.

Dr. R. S. Scorer : Sunrise and sunset are the best times for a large-amplitude disturbance at high levels because the static stability in the lowest layers is larger than by day and the surface wind is stronger than at night. On the other hand I have often seen cirrus that was very probably of orographic origin at all times during the day. On 27 September last there had been much cumulonimbus forming false cirrus during the day, and in the evening very similar cloud appeared out of clear sky and streamed away from a fixed point for about an hour : it would certainly have been reported as false cirrus. It was forming over the Cheltenham region, among other places.

Mr. E. Gold : Dr. Scorer has dealt partly with a question I wished to ask, namely why these clouds should occur near sunset and sunrise. If they were caused by waves in the general current set up by the mountain, one would expect them to occur at other times. Did radiation play a part in causing or intensifying them near sunset or sunrise ? One would expect that the effects at sunrise would be in the opposite direction to those at sunset. Mr. Ludlam said the clouds occurred at a level where the temperature was — 40°F or C, were shallow, and did not occur at temperatures of — 50°C. Could he say what was the basis of his statement that the cloud was shallow ? Was he satisfied that it did not in fact extend upwards to levels where the temperature was — 50°C or less ?

Dr. J. S. Farquharson : It seems to me difficult to understand why the orographic influence of the Welsh mountains should not have caused the formation of widespread cirrus in a westerly airstream if cirrus formed a little to the east of these mountains owing to the orographic influence of hills. Barrett, Herndon and Carter (*Tellus*, **2**, 1950, p. 302) have suggested that a saturated layer of the atmosphere is a typical feature of the lower stratosphere in America and recent observations in this country (*Met. Mag.*, **81**, p. 341) support the idea that such a layer can be observed at times. In these circumstances it is surprising that orographic cirrus is not observed far more commonly than apparently it is, if it can be caused by relatively insignificant topographical features.

Mr. J. S. Sawyer : The examples of orographic cirrus given by Mr. Ludlam occurred with light or moderate winds in the lower atmosphere. Is this typical ? One might have expected the best examples with stronger winds.

Mr. F. H. Ludlam (*in reply*) : The hill-shower observed by Mr. Bonacina was almost certainly, as he suspected, falling from ' false cirrus ' which was the last remnant of a cumulo-nimbus. I do not think orographic cirrus has any significant effect on the release of rainfall, although it is conceivable that it might sometimes be helpful.

Mr. Schove suggests that all our cirrus is associated with fronts, and while I would agree that orographic effects are most commonly seen in frontal-cirrus systems I think that sometimes they cause the appearance of cirrus which would not otherwise have formed. This bears also on Dr. Farquharson's comments ; no phenomenon is observed more often than it is looked for, and I find it hard to escape associating orographic features with many of the cirrus clouds which I see. Dynamical meteorologists who study mountain effects agree that these are not merely proportional to the height of the mountains, and although it is a comfort to find that they are not perturbed at the idea of cirrus forming over small hills rather than nearby bigger ones, I would in any case put more faith in the evidence of the observations.

Dr. Scorer has answered Mr. Gold's first question. Of course I can only guess the thickness of cirrus clouds, and when I say that they are shallow I mean only that they look thin or 'flat.' Certainly the first-formed details are often less than a thousand feet thick, appearing as very slender strips with well-defined upper surfaces.

In reply to Mr. Sawyer, I have too few detailed observations of orographic cirrus to be sure of what is typical in the wind-field, but according to the theorists the hill effects are most pronounced when there is a strong increase of wind with height, and so may well often occur with rather light winds in the lower atmosphere.

A study of the rainfall of two synoptic situations

By J. S. SAWYER

(Read 19 November 1952. See *Q.J.*, **78**, p. 231)

Mr. G. B. Tucker : Although the purpose of Mr. Sawyer's paper was not to analyse in detail the pattern of orographic rainfall, I should like to state the results of some preliminary calculations of a more detailed nature which in general corroborate some of his conclusions.

Applying Dr. Scorer's solution of perturbation equations for an air-mass with uniform velocity shear in the vertical, flowing over a certain shaped two-dimensional ridge (1 km in height), I have calculated the resulting streamlines. Since a general solution is unobtainable this was regarded as being a realistic case in that the streamlines closely resemble those for a uniform air mass flowing over a conical hill. The air was assumed to have a static stability equal to that of a dry air mass whose vertical temperature profile followed the 60°F saturated adiabatic. The extreme solution, with the wave crests at their maximum displacement down-stream, was taken – this solution also gives the maximum vertical velocities over the ridge. From these streamlines, fields of horizontal and vertical velocity, and added water content were calculated assuming the air mass to be initially saturated and no water to be precipitated.

Introducing particles of 100 μ and 1,000 μ at various heights along the isopleth of zero water-content (i.e., at the leading edge of the orographic cloud), I computed their fall trajectories by means of a step-by-step integration of the well-known equations, taking the efficiency of catch as unity. Also, from the field of vertical velocity I calculated the rate at which water is liberated in an unit column.

Considering first the drizzle droplet, the results imply that if it is drizzling in the undisturbed airstream, then over the mountain area the droplets will, in general, have no time to grow into raindrops, except perhaps in the heavy drizzle band indicated by the sample trajectory in Fig. 1; here the five-fold increase in radius 5 or 6 km to the lee of the crest denotes the area of maximum increment to the normal rainfall. Thus in this case moisture produced by uplift cannot be brought down to the ground at the rate at which it is made available. If under these conditions, heavy rainfall is experienced on the hill, we must postulate some process other than simple orographic uplift to account for it.

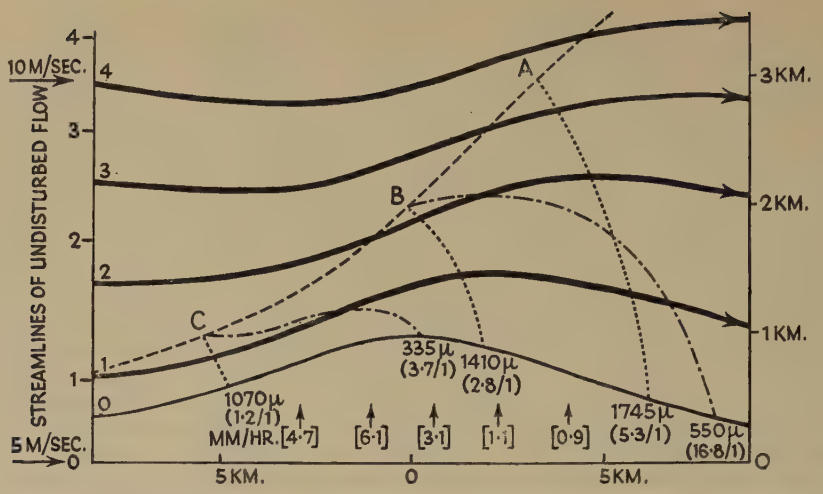

Figure 1. Thick lines are streamlines. The dashed line denotes the position at which the streamlines regain their original level, and beyond which water is condensed out. Dotted lines are sample trajectories of original 1,000 μ droplets, and dot-dashed lines of original 100 μ droplets. Where these reach the ground the final radius is indicated, and also the ratio of the final to the original volume (in round brackets). The figures in square brackets indicate the rate in mm/hr at which water is being condensed out in the unit vertical column.

However in the situation examined by Mr. Sawyer there was general rainfall in the area, and here, as we can see from the 1,000 μ drops (Fig. 1), is a mechanism by which most of the orographically liberated moisture can be brought to the ground almost vertically. If we assume that the rate of rainfall is proportional to the size of the drops (an assumption not generally justifiable, but here serving our purpose), then an increase of up to five times the undisturbed rainfall is implied. The trajectories and radius increments were however computed from water contents which assumed no precipitation, and thus substantial modifications are necessary, but these will be such as to bring the maximum nearer the crest.

Assuming a general rate of 1 mm/hr, we see that the rate implied by drop growth exceeds the rate of liberation in the column above. The drop, therefore, as it falls, drains from each level the residual moisture which could not be gathered by drops upwind. The process thus becomes rather complex, but we can make the rough estimate that the area of maximum increment to normal fall will be about 4 km to the lee of the crest, and the rate will be about 4 or 5 mm/hr. Modifications for drops of different sizes will tend to push the maximum towards the crest because the mean size of drops at ' C ' will probably be greater than those at 'A' (Fig. 1).

Downwind of the wave crest the air descends, but the water condensed from it will have been precipitated, hence the normal fall descends through an unsaturated layer.

Thus even in this extreme solution chosen, the belt of maximum increment occurs no further than 4 or 5 km to the lee of the crest, and if it is observed elsewhere we must postulate another mechanism to account for it. When considering drizzle or actual rain, the advent of convective processes and cumulus development within the orographic layer presents itself as a plausible solution. Despite the fact that we have taken rather light winds the results are in general agreement with those of Mr. Sawyer, both in that the increment is of the right order and that ' the orographic component occurs primarily within the rain areas already established by dynamical causes.'

Dr. J. M. Stagg : Mr. Sawyer's paper is an important contribution to synoptic meteorology : I believe it is the first time that both the distribution and the amount of rain from a depression have been computed dynamically. An experienced forecaster might have made equally good or even better assessments without the computations, but in the long view that is of less importance than the fact that dynamical theory can already give a fair answer without the interposition of human judgment.

The forecaster will want to know how long he may have to wait before Mr. Sawyer's procedure, based on Dr. Sutcliffe's development ideas, can be introduced into his daily practice and applied to his prebaratic and prontour charts : he will also want to know how long the actual procedure takes. From Figs. 1a and 1b in Mr. Sawyer's paper it looks as if the thermal development term made only a small contribution to the relative divergence, so in the case of that particular depression

at least a forecaster with a good surface prebaratic should have been able to make a fair estimate of the distribution of rain from his prebaratic alone using the Sutcliffe-Sawyer ideas. With the further extension in this paper it would also have been practicable to estimate the total amount of rain from the prebaratic alone. But this short cut would be practicable only when the thermal field is approximately uniform and straight.

If this line of thought is right it raises the question why the ζ' term which is generally regarded as the major development term in the Sutcliffe expression makes only a small contribution in comparison with the ζ_0 term which I have come to regard primarily as the steering element in the total development.

Dr. R. S. Scorer : It is not evident that it is appropriate to apply Bushby's correction factor of 4 for the static stability on an occasion when the stability may have been very small owing to saturation of the air.

Most interesting is the absence of orographic rain over the Pennines : this, I think, is due to the variation in wind direction with height, not simply the small surface wind. The greatest orographic effects are produced when a ' solid ' wind current crosses a mountain.

From the paper one was led to believe that Mr. Sawyer was attempting to calculate the height of the surface of no orographic disturbance, but in his presentation this was not apparent. In fact the model of the flow pattern does not seem to make a great deal of difference because the added water comes mostly from the lowest layers where the flow conforms, though rather roughly, to the mountain. If the flow is not laminar we do not know what happens, and it is possible to show that if a layer in middle levels became unstable (or even statically neutral), through condensation for instance, the flow could not continue to be laminar and steady. There may be some overturning under these circumstances and large cumulus embedded in the cloud system, and perhaps the outbursts of rain at Chivenor is evidence of this : could Mr. Sawyer comment on that rain ?

Communicated : It is notable that Mr. Sawyer, in separating the developmental and orographic effects, assumes that the orographic effect is unaffected by the earth's rotation and that it does not affect the process of development. Though this may be true of a vigorous frontal depression over the British Isles it would not be so true of Scandinavia, or Spain, or Italy, or indeed any place where depressions are guided or otherwise affected by the geography.

Prof. P. A. Sheppard : This is a notable paper and one could have wished L. F. Richardson to have been present to see how weather prediction by numerical process is being tackled by a younger generation of meteorologists. The fact that the paper describes ' retrospective prediction ' is almost irrelevant in the major problem of quantitative synoptic meteorology. So, too, is the question of how long it took the author to make his calculations of rainfall rates and amounts; speed here is now perhaps largely a matter of engineering.

I wish to ask the author a question relating to scale. The dynamical meteorologists active in the numerical forecasting problem are necessarily concerned with the large-scale flow pattern, not least because of the generally wide separation of upper-air stations, but also presumably because of small-scale variations in the damping factor. The author has however here dealt with a rather small-scale problem, though in a region where the upper-air network is a good deal closer than usual. Would he indicate whether he believes the sort of problem discussed in his paper can be tackled within the framework of the large-scale prediction problem or whether it is essentially a fresh problem which nevertheless would be amenable to routine numerical methods ?

Dr. J. S. Farquharson : I have performed some computations of the Sutcliffe development term relative to a particular situation but, using the thicknesses of the 1,000 mb-700 mb, 1,000 mb-500 mb and 1,000 mb-300 mb layers. At a given time the area of maximum development varies in position and magnitude with the thickness. It would appear that the best thickness for the computation would depend on the scale of the depression and on the size of the vertical velocities in it. It is not self-evident that the thickness 1,000 mb-500 mb is always or even usually the best thickness for the computations carried out in this paper.

Mr. Sawyer also multiplies the development term by a factor of 2 on the assumption that this gives a measure of the development up to the tropopause. This assumption while convenient for purposes of computation seems to me difficult to justify on physical grounds and it is surprising that with such an assumption the result appears as good as it does.

Mr. L. C. W. Bonacina (*communicated*) : I regret only to have heard the opening of the discussion which, however, bore upon a point of much interest to me. Mr. Sawyer, I gather, found that the orographic rain, though more intense, was not of longer duration than that which fell in the low country.

Coming to mean annual values I have often wondered whether there is any spot in the British Isles where it rains for half the time. Such few data as exist suggest that a quarter of the time is about the limit – which is quite bad enough. Thus while London's 25 in. only occupies 6 per cent of the time, Snowdon's 200 in. does not occupy 48 per cent of the time but only 24 because the rate of fall is double.

Mr. J. S. Sawyer (*in reply*) : In reply to Dr. Stagg I can say that calculations of the probable dynamical and orographic components of rainfall have been made by the methods of Sections 2 (*e*) and 2 (*f*) of the present paper but using prognostic charts. The uncertainties of the forecast chart justify only a very rough calculation, and this can be done in about an hour. I might add that, if the forecast chart is a good one, the results give a picture of the distribution of rain reasonably correct in broad outline, but are probably not better than could be constructed from subjective judgment.

As Dr. Stagg points out, the main contribution to the calculated 'development' arose from the second term in the expression at the top of p. 238 (viz. $-2V'\partial\zeta_0/\partial s$). This is usually so; the so-called 'thermal steering term' (the ζ_0 term) is of greater importance in determining the vertical motion than is the 'thermal development' term (the ζ' term). The importance of the latter arises from its ability to give rise to new cyclonic and anticyclonic centres, whereas the 'ζ_0 term' cannot.

As Mr. Tucker and Dr. Scorer have indicated, the mechanism by which orographic rainfall is produced is necessarily complicated. I, also, had regarded the short periods of heavy rain at Chivenor and at other places in the west of England as due to the development of vertical instability, although not necessarily through any very deep layer of the atmosphere.

In reply to Prof. Sheppard I should like to refer to the work which Mr. F. H. Bushby and I have been doing in an attempt to carry out numerical integrations of some of the equations of large-scale atmospheric flow. In deciding on the distance between the individual points of the network to be employed in our calculations, we have been limited to a length of between 150 mi and 200 mi. The wide separation of upper-air stations would not justify a smaller grid length, whereas use of a larger grid length would cause us to omit features which, although of relatively small scale, appear to us to have an important influence on the atmospheric motion even on the largest scale and which cannot properly be taken into account by averaging over an area. Thus it is my opinion that when numerical methods are employed in the dynamics of the synoptic problem, it will be necessary to treat together all motions on a scale larger than 200 mi or 300 mi. We cannot however hope that details of less extent than 200 mi will be adequately represented, and the vertical motion and pressure changes accompanying individual fronts will necessarily be 'blurred.'

Evening waves

By R. D. ROPER

(Read 17 December 1952. See *Q.J.*, **78**, p. 415)

Dr. R. S. Scorer : After the National Gliding Contests at Camphill in July 1951 a competition was held in which glider pilots were invited to describe some meteorological phenomenon. Sir Simon Marks generously presented some prize money, and Mr. Roper's paper gained the first prize. Other prizewinning entries by A. J. Fyfe, G. O. Smith and G. H. Lee and O. W. Neumark have been published in *Weather*, *Gliding*, and the *Meteorological Magazine* respectively. Papers of this kind are of great value to meteorologists because they give information of a kind not normally available.

It must not be thought that the flights described by Mr. Roper and others, for instance the flights from the Long Mynd in Shropshire on November last, in which several gliders ascended to over 10,000 ft, represent in any way the limit to the possibilities. Pilots are generally not prepared for cold, they do not usually have oxygen, and often flights are cut short by the onset of darkness. I see no reason why a glider should not soon soar to 20,000 ft or more in a wave in this country, for both the theory and evidence from clouds suggests this to be a distinct possibility.

The information gained from these flights is of great value in informing us of the way air currents flow over mountains, it supplies us with a sound basis for theory, and we are now in a position to advise the pilot of the powered aircraft what to do when large areas of lift or sink are found. Aircraft being flown by the automatic pilot have been known to ascend in waves over France as much as 7,000 ft in 7 min. This might have been disastrous to an unwary pilot if it had been sink in the other side of the wave, or if the aircraft had not been pressurized and had no oxygen. Many mysterious crashes of aircraft into mountain sides have been ascribed to bad navigation and non-compliance with safety height regulations, and for safety's sake alone the pilot should know what the possibilities are and how to act.

Prof. G. Manley : I should like to ask Dr. Scorer whether the likelihood of exceptional wave development might be forecast for certain areas. It seems evident that, with wind of a particular direction and speed, certain valleys would be likely to impose or force waves much better than others, depending on their width and orientation in relation to the speed and depth of the surface-wind current. Can you get a sort of resonance effect, for example, in places where the landscape presents not one, but a succession of corrugations ?

Mr. A. H. Yates : There is now no doubt that atmospheric waves such as Mr. Roper has described are very common. They have been found in Great Britain near to most of the gliding sites and probably would be found elsewhere if facilities for glider launching were available. Powered aircraft have explored wave upcurrents over Britain up to 30,000 ft and in the U.S.A. gliders have reached 45,000 ft.

Dr. Scorer has shown that a condition required for the existence of these lee waves is an increase of wind speed with height. On the day of the Dakota crash near Snowdon in Jan. 1952 the wind increased at one time from ' calm ' at the ground to 150 kt at 30,000 ft and it was quite possible that the ill-fated aircraft met the severe downcurrents of a lee wave. In the light of this knowledge it is obviously inadequate to specify safety heights of one thousand feet above the hill crests and it is thus important that the safety heights be increased (as recommended by the inquiry assessors) and that pilots be encouraged to study the findings of the meteorologists and glider pilots so that they can, themselves, add to our knowledge of these waves.

Mr. E. Gold : I would like to ask if it is possible to differentiate beforehand between those occasions when there will be evening waves and the occasions when evening waves are not to be expected.

Mr. G. R. Veryard : Am I right in inferring from Mr. Roper's illustrations that the ' evening waves ' are not necessarily an evening phenomenon but, given suitable conditions, may occur at any time of the day ? Is there a critical wind speed above which, instead of waves being produced, the airflow over the hills becomes generally turbulent or chaotic ?

Prof. G. C. Varley : I wonder if the main features of the evening waves are that they arise independently of cloud formation, and are therefore more difficult to study. There would seem to be advantages in investigating waves in places like Hartside, where the conformation of the hill is relatively simple.

The various accounts of wave phenomena differ in their observations of surface wind, and in view of Prof. Manley's descriptions of the Helm wind, this may be due to purely local effects in relation to the wave.

Dr. R. S. Scorer (*in reply*) : Mr. Gold is correct in saying that when waves are strong they can usually be foreseen, and when they will definitely not occur it is generally possible to say so, but without more experience the cases in between are necessarily difficult. The ordinary forecaster cannot be expected to do much at present because he does not have the information necessary for learning the technique. No routine reports are made when waves occur; the cloud-reporting codes are not designed to describe stationary wave clouds and pilots are not encouraged to report waves partly because they do not enter into their briefing. Professor Manley would like us to forecast the wavelength : it is almost certainly easier to observe the waves and make deductions about the airstream; the waves themselves should be regarded as a primary observed phenomenon like any other meteorological element.

At present there is no basic theory to deal with the flow when large eddies are formed, though Forchtgött has described some of the characteristics. Clearly more research is needed now that waves are more or less understood.

Mr. R. D. Roper (*in reply*) : I find very interesting Dr. Scorer's and Mr. Yates's remarks concerning the effect of standing waves on powered aircraft. The strength of the downdraught in the lee of a well-developed standing wave is, perhaps, not sufficiently appreciated. A case has occurred (11 March 1950) of a sailplane flying in the upcurrent of a standing wave, over the Bradwell valley in Derbyshire, in the lower levels of which the wind speed was 60 to 70 mi/hr.

The rate of lift given to the sailplane was in excess of the maximum that could be recorded by the rate of the climb indicator, the maximum being 1,200 ft/min. Making due allowance for the rate of sink of the sailplane at 70 mi/hr air speed, the corresponding downdraught area in the rear of the wave must have achieved a rate of descent in excess of 1,800 ft/min.

In reply to Mr. Veryard's and Prof. Varley's remarks concerning the significance of the term, ' evening waves,' I would emphasize that this was used primarily as an amendment to the term ' evening thermals,' which implies, obviously, a convective phenomenon. In essentials, the evening wave is very little different to the normal standing wave, illustrations of which were shown. The differences that do exist arise out of the circumstances surrounding their development. For example : a common type of wave that occurs in the daytime arises in an already suitably conditioned air system overlying a shallow, convective layer near to the ground. The glider pilot makes use of either hill lift, or hill lift and convection, in order to reach the layer in which the wave is operating.

The circumstances surrounding the development of an evening wave differ in that the rapid change of lapse rate in the evening produces a layer near to the ground which is favourable for wave formation; the depth of this layer progressively increasing as the cooling is distributed. Under these conditions an upwind movement of the lift area has been noticed, suggesting the possibility that as the stability increased and the stable layer deepended, so the wavelength shortened. Also, the formation of clouds does not take place until a late hour in the evening, if at all. Hence the difficulty of study, observation and photography. On occasions when cloud does form, the appearance is similar to the usual daytime standing wave cloud.

The first type of wave can, of course, appear in the day and carry on throughout the evening, but I only know of one occasion when an evening wave may have occurred during the daytime.

Observations of the westerlies over the sea

By P. A. SHEPPARD, H. CHARNOCK and J. R. D. FRANCIS

The wind stress over the ocean from observations in the Trades

By P. A. SHEPPARD and M. H. OMAR

(Read 17 December 1952. See *Q.J.*, **78**, Pp. 563 and 583)

Mr. J. S. Sawyer : I would like to ask whether the turbulence described in Section 8 of the paper is really the main agency in the vertical transport of momentum. If smaller-scale eddies are more important in this process, it cannot be argued that the constancy of turbulence intensity with height necessarily implies the constancy of shear-stress.

Dr. R. C. Sutcliffe : It is interesting to recall that Dr. Dobson's early estimates of surface friction from pilot-balloon ascents appeared in 1914. Until 1936 when I made new estimates very little more had been published – and now it is 1952. We must be grateful to Professor Sheppard for tackling this fundamental problem once more. In my work there seemed little difficulty in obtaining reasonable estimates over the land as there were large numbers of pilot-balloon results and by picking out cases with nearly solid currents above the friction layer the effect of the upper levels was presumably largely eliminated. Over the sea, data were few and the processes discussed by Professor Sheppard probably account for the remarkably small values I obtained for the surface friction.

The forecaster will note the small mean deviation of surface wind from gradient-wind direction with westerly winds over the sea and should assume as a first approximation that the run of the isobars is along the direction of the reported winds from ships. Cases vary however and it may be possible to make allowances for differences in the variations of winds with height in the free atmosphere.

The results are also important in the problem of the general circulation. It is fairly well established that westerly angular momentum is transferred into the upper westerlies largely by ' synoptic turbulence ' at high levels from lower latitudes. According to Professor Sheppard's argument this may be diffused down to the surface frictional sink by small-scale turbulence. There is however no corresponding link between the surface frictional source in the trade-wind zone and the upper levels (if there is no diffusion through the levels of maximum easterly component) and we require another mechanism to complete the circuit. Perhaps Dr. Eady will have something to say about this. Deep penetrative convection in the equatorial zone is presumably the required link with some meridional circulation in the vertical plane.

Mr. E. Gold : I should like to congratulate the authors on this very valuable contribution to our knowledge of the variations of wind in the lower layers over the sea. May I ask if there were simultaneous observations of temperature at short intervals and if the rather substantial changes of wind were accompanied by appreciable changes of temperature ? May I also ask if Professor Sheppard is satisfied that the deduction from the records in the trade-wind region is the sole explanation of the results ? Presumably records of any occasion in temperate latitudes when the thermal wind was opposed to the surface wind should show the same results.

Mr. L. C. W. Bonacina : When I heard Professor Sheppard refer to the climatological bearing of his work I began to wonder about the mechanism by which air currents take up the temperature of the ocean currents over which they blow and which they largely control. I have, however, gathered from him that the problem of heat transfer is not directly involved in his paper.

Dr. J. M. Stagg : Do these observations throw any light on the abrupt change of surface drag over the ocean at wind speeds of about 6 m/s, as reported by Rossby and Munk ?

Mr. C. H. B. Priestley (*communicated*) : The paper by Sheppard and Omar contains data of much interest and value, but I would like to discuss one of the hidden assumptions which must throw some doubt on the accuracy of values of surface stress and drag coefficient obtained by this method. The authors have taken such precautions as they can to exclude the effects of acceleration terms such as $u\dfrac{\partial u}{\partial x}$ and $v\dfrac{\partial v}{\partial y}$ but there can be no corresponding assurance about the $w\dfrac{\partial u}{\partial z}$ term so that one would have preferred to see Eq. (1) written more fully as

$$\tau_0 = -l\int_0^h \rho\,(v_J - v)\,dz - \int_0^h \rho w\frac{\partial u}{\partial z}\,dz$$

From figures supplied in the paper, calculation shows that a w of only 1 cm/sec will generally be enough to make the third term comparable with the second. With such fine tolerance it might at first be doubted whether even 20 to 40 results at the same wind speed would be sufficient to reduce the error of the mean within acceptable limits. However the notable regularity in the values of c in Table II (San Juan excepted) gives some reassurance that random errors have been adequately suppressed. Whether systematic errors are eliminated is another matter, for this requires further that w has a true mean of zero *within each group* of wind speeds, i.e., that w in the lowest 2,000 ft is completely uncorrelated with surface-wind speed u.

Though within the framework of small-scale turbulence this might at first seem a reasonable enough assumption, it appears less so from the point of view of the angular momentum balance in the general circulation. The correlation between w and u (now written for easterly component of wind) represents the upward eddy transfer of zonal momentum effected by the synoptic disturbances of these latitudes. Although there do exist other means of effecting this transport, the total neglect of the synoptic scale disturbances in this role would amount to an extreme hypothesis.

Adopting a less extreme point of view, we may use what is known from recent work on the larger-scale problem to provide some indication of the sign and order of magnitude which this correlation may have, and so of the possible correction to the τ_0 values derived by Sheppard and Omar. To do this we might reasonably suppose that a considerable part (say one-half, in the absence of any better information) of the angular momentum carried polewards by disturbances at high levels across latitudes $\pm 30°$ is brought to those levels from its source at the surface in the intervening latitudes by disturbances of similar and rather smaller, though still comparable size. (This supposition does not deny an important role also to the mean meridional circulation). The magnitude of the lateral transport is known with fair accuracy; I have assessed it as equivalent to $1{\cdot}1 \times 10^8$ dyne/cm, Starr and White some 50 per cent higher. Taking then $1{\cdot}4 \times 10^8$ as an approximate figure, the mean upward flux due to the synoptic scale of disturbances

will be around 0·25 dyne/cm²; with 3 m/sec as the local standard deviation of E-component of surface wind in the Trades, the pattern of regression (assumed linear) of w on u at the surface would be as under :

u (kt)	=	5	9	13	17	21
w (cm/sec)	=	+ 0·9	+ 0·5	0	− 0·5	− 0·9

The last term in the equation above is equivalent to $-\rho w \Delta u$, where Δu is the increase in u from the surface to the level h. Sheppard and Omar's results indicate that Δu is generally about 4 kt for the lighter and 6 kt for the stronger winds. On this basis, therefore, it would appear that their values for τ_0 may be subject to a systematic error, being overestimated by some 0·25 dyne/cm² at 5 kt and underestimated by 0·4 dyne/cm² at 21 kt.

I do not wish to claim that these figures are any more than indicative, but they do bring out one or two points of rather special interest. They serve to draw our attention to the intimate connection between the largest- and smallest-scale problems and the possibility that the former might even contribute towards the quantitative solution of the latter : the correction suggested here is in the sense that would restore the increase of drag coefficient, c, with wind speed which has been indicated by a number of other workers; it is implied that convection in the Trades may be correlated (albeit only slightly) with easterly wind speed, a point deserving closer examination. The corrected equation also suggests that the τ_0 values derived for San Juan may not be due entirely to a real increase in drag coefficient, but partly at least to the neglect of a systematic upward velocity caused by the relief of the island.

Prof. P. A. Sheppard (*in reply*) : In reply to Mr. Sawyer, we have no categorical evidence that the turbulence of moderate scale (order 10 min to a few hours) is solely or even mainly responsible for the downward transport of momentum from higher levels – the observations of the vertical velocity of the air which would be needed to test this hypothesis are unfortunately not available. Nevertheless we regard the hypothesis as reasonable in view of the reversals of velocity gradient and of variation of wind direction with height given in Fig. 4 and Table I.

Dr. Sutcliffe and Mr. Gold have both made the point that evidence for a change in the nature of the near-surface flow with respect to the isobar according to the alignment or opposition of surface and thermal winds should be available without going to the Trades. We have in fact begun such an investigation but it is not yet complete. To the other point raised by Mr. Gold, sea and air temperatures were observed at Scilly and are given in the Appendix to the first paper but no obvious relation with the wind profiles was evident.

Dr. Sutcliffe, Dr. Eady and Mr. Priestley have referred to the significance of our work in relation to problems of the general circulation. I hope to elaborate on that in a later paper and so will avoid detail here. We should have made it clearer in our paper on the Trades that the quantities we have listed in Table II under τ_0 are more properly the differences of shear stress between the surface and the level of maximum wind. We thought it reasonable as a first step to assume zero transfer across the latter level since, for the first time, reasonable values of ocean stress were then obtainable from the geostrophic departures, and secondly because we considered it rather unlikely that large-scale mean vertical motion of significant amount would develop over so thin a layer. The question should evidently be investigated but the difficulties are apparent. At higher levels it is more likely that mean vertical motions, whether from synoptic disturbances or as a climatological feature, are involved. This view implies the neglect of transfer across the level of maximum wind by means of small-scale eddying, an assumption which also requires test.

Whatever may be the truth about momentum transfer at the level of maximum wind, it is worth emphasizing that the angular momentum balance can be satisfied without recourse to such transfer. A meridional circulation is then of course fundamental to the mechanism, as indicated by Dr. Sutcliffe and Mr. Priestley. Moreover until clear evidence has been produced of transfer of momentum *against* the gradient in the vertical, it seems reasonable to see how far other processes, of which we have clear evidence, will carry us. That is some way, I suggest. There is perhaps a danger of eddy transfer on the synoptic scale being overworked – its importance in meridional transfer is not in question – but since there *is* a meridional circulation in lower latitudes destruction of westerly momentum below and its creation aloft *must* be allowed for. We have shown how the former may be accounted for.

If it turns out that there is a transfer of momentum across the level of maximum easterly which is dependent on surface-wind speed, as suggested by Mr. Priestley, then a variation of drag coefficient with this parameter which does not appear in our results and which Dr. Stagg has looked for, may re-emerge.

Proceedings at meetings of the Society

Popular Lecture, 11 *December* 1952

Wg. Cdr. R. M. Poulter, O.B.E., being in the chair, Dr. G. M. B. Dobson, C.B.E., F.R.S., gave the annual popular lecture on ' The upper atmosphere – from 10 to 100 miles up,' in the Beveridge Hall, University of London. The lecture was open to sixth-form students, of whom some 500 were present by arrangement with the education authorities of the London, Buckinghamshire, Essex, Hertfordshire, Kent, Middlesex and Surrey County Councils.

Ordinary Meeting, 17 *December* 1952

The following candidates were declared to have been elected that day by the Council :

Fellows

Beaver, Prof. Stanley H., M.A., F.G.S., F.R.G.S., University College of North Staffordshire, Keele, Staffs.

Bridges, Richard, 12 Meadows Road, Heaton Chapel, Stockport, Cheshire.

Browne, Ian Campbell, M.A., Ph.D., University of Manchester, Jodrell Bank Experimental Station, Holmes Chapel, Nr. Crewe, Cheshire (*transfer from Student Membership*).

Cochrane, Oswald Collins, c/o B.O.A.C., Sanshin Building, Yuraki-cho, Tokyo, Japan.

Cole, Monica Mary, B.Sc., Ph.D., University College of North Staffordshire, Keele, Staffs.

Curtis, Allan Raymond, M.A., University of Sheffield, West Bank, Sheffield, 10.

Davies, David John, 17 Holyrood Avenue, South Harrow, Middlesex.

Deadman, Capt. Kenneth Charles, 9 Elmfield West, Millbrook Road, Southampton, Hants.

Fordham, Joan Patricia, B.Sc., 3 Forest Gate, Kingsbury, London, N.W.9

Fotheringham, Instr. Lt. Richard Richardson, R.N., 60 Beechwood Road, Sanderstead, Surrey (*transfer from Student Membership*).

Gibbs, William James, M.Sc., S.M., Research Section, Meteorological Branch, Box 1289K, Melbourne, Victoria, Australia.

Gilchrist, Bruce, Ph.D., A.R.C.S., D.I.C., Electronic Computor Project, Institute for Advanced Study, Princeton, N. J., U.S.A. (*transfer from Student Membership*).

Johnson, Derek Henry, M.Sc., A.R.C.S., D.I.C., c/o Forecasting Research Division, Meteorological Office, Dunstable, Beds (*transfer from student Membership*).

McMahon, Thomas, 9 Camp Road, Sudbrook, Nr. Chepstow, Mon.

Saxby, Thomas Eric, B.Sc., 132 Finedon Road, Irthlingborough, Wellingborough, Northants.

Triegaardt, Daan Odendaal, B.Sc., Weather Office, P.O. Box 584, Bloomfontein, South Africa.

Foreign Members

Cramer, Harrison E., A.B., S.M., Sc.D., Round Hill, South Dartmouth, Mass., U.S.A.

El-nady, Salama Saleh Abd., B.Sc., 38 Queen's Gate Terrace, London, S.W.7.

El-salam, Mahmoud Samy Mahmoud Abd., B.Sc., 69 Cornwall Gardens, London, S.W.7.

Hatakeyama, Hisanao, D.Sc., Director Meteorological Research Insititute, Mabashi, Suginami-ku, Tokyo, Japan.

Isono, Asst. Prof. Kenji, B.Sc., Geophysical Institute, Tokyo University, Tokyo, Japan.

Kodaira, Joshio, D.Sc., Director, Tokyo District Central Meteorological Observatory, 7, 1-chome, Ote-machi, Chiyoda-ku, Tokyo, Japan.

Koko, Maung, 11 Sydney Crescent, Hamble, Hants.

Ogura, Yoshimitsu, B.Sc., Geophysical Institute, Tokyo University, Tokyo, Japan.

Record, Frank A., A.B., S.M., Sc.D., Round Hill, South Dartmouth, Mass, U.S.A.

Sakuraba, Shinichi, Librarian, Central Meteorological Observatory, 7, 1-chome, Ote-machi, Chiyoda-ku, Tokyo, Japan.

Syono, Prof. Sigekata, D.Sc., Geophysical Institute, Tokyo University, Tokyo, Japan.

Takahashi, Coichiro, D.Sc., Forecasting Laboratory, Meteorological Research Institute, Central Meteorological Observatory, Takehira-cho, Chiyoda-ku, Tokyo, Japan.

Takahashi, Yoshihiko, D.Sc., Meteorological Research Institute, Mabashi, Suginami-ku, Tokyo, Japan.

Wadati, Kiyoo, D.Sc., Director, Central Meteorological Observatory, 7, 1-chome, Chiyoda-ku, Tokyo, Japan.

Yamamoto, Prof. Giichi, D.Sc., Geophysical Institute, Tohoku University, Katahira-Cho, Sendai, Japan.

Associate

Jones, Dennis, B.Sc., London House, Guilford Street, London, W.C.1.

Student Members

Blydenstein, Elisabeth R., Imperial College Hostel, Prince Consort Road, London, S.W.7.
Robinson, John Godfrey, 'Hillview,' Bucklebury, Nr. Reading, Berks.
Scott, William Porteous, 17 Hope Park Terrace, Edinburgh, 8.

The following papers were read and discussed :

(1) 'Evening Waves' by R. D. Roper (*Q.J.*, **78**, p. 415).

(2) (*a*) 'Observations of the westerlies over the sea' by P. A. Sheppard, H. Charnock, and J. R. D. Francis (*Q.J.*, **78**, p. 563).

(*b*) 'The wind stress over the ocean from observations in the Trades' by P. A. Sheppard and M. H. Omar (*Q.J.*, **78**, p. 583).

Joint Discussion Meeting, 21 January 1953

The meeting on 21 January took the form of a joint discussion meeting with the British Gliding Association on 'Fair weather cumulus,' and was held in the Lecture Theatre of the Science Museum. Mrs. Ann Douglas, a Councillor of the Society and Vice-Chairman of the British Gliding Association, was in the chair and there were some 170 Members and visitors present. The opening speakers were :

Mr. A. H. Yates (Royal College of Aeronautics) : 'The nature of thermals.'
Dr. D. G. James (Meteorological Research Flight) : 'Fluctuations of temperature below cumulus cloud.'
Mr. L. Welch (Miles Aircraft and Surrey Gliding Club) : 'What gliders find and their potentialities for research.'

After a general discussion, and the showing by Mr. F. H. Ludlam of films of cumulus clouds, there was a summing up by Dr. R. S. Scorer.

Special General Meeting, 18 February 1953

A Special General Meeting was held to consider the following amendments proposed by Council to the by-laws :

I.

1. By-Law 41. Reword as follows :
'There shall be a President, four Vice-Presidents together with a Vice-President for Scotland and one additional Vice-President for each Oversea Branch, a Treasurer, and up to four Secretaries, two of whom shall perform the duties of Editor and Librarian respectively.'

2. By-Law 42. Delete lines, 5, 6 and 7 and substitute :
'The Treasurer;
The Secretaries;'

3. By-Law 47. Delete.

4. By-Law 63. Line 1 delete 'appoint a Librarian and shall'.

II.

1. By-Law 49. Line 1, substitute '1st' for '31st'.

2. By-Law 50. Line 1, substitute '14' for '21'.

3. By-Law 51.
   (a) Line 1, substitute 'June' for 'July'.
   (b) Reword last sentence on page 15 as follows:
   'Provided that if no candidate has been nominated under the provisions of By-Law 50, the persons nominated by the Council shall be declared by the President at the Special General Meeting (called for the purpose) to be elected unopposed and no ballot papers shall be issued'.
   (*c*) Delete second paragraph.
   (*d*) Reword third paragraph as follows:
   'In the event of there being a ballot each Fellow voting shall erase sufficient names to reduce the names on the ballot papers after such erasure to the number to be elected to the respective offices. The ballot papers shall be returned to the office of The Society by post or by hand so as to reach the Honorary Secretaries at a Special General Meeting held to elect the Council'.
   'At this General Meeting the Fellows present shall choose two or more scrutineers for the purpose of the ballot'.
   (*e*) Line 2, fourth paragraph, after 'them' insert 'at the said General Meeting'.

4. By-Law 65. Reword sub-section (c) as follows:
   'Special General Meetings of Fellows for the purpose of electing the Council of The Society, or for revoking, altering or amending the By-Laws of The Society, or for any other specific purpose'.
   Delete sub-section (d).

5. By-Law 70. Line 4, delete 'to appoint two or more Scrutineers'.

The amendments were adopted.

Ordinary Meeting, 18 February 1953

The following candidates were declared to have been elected on 21 January 1953 by the Council:

Fellows

Davis, Maurice, M.Sc., 41 Southwood Avenue, London, N.6. (*transfer from Student Membership*).
Earle, Percy Algernon, B.Sc. (Econ.), 82 Ashford Road, Iver Heath, Iver, Bucks.
Jarman, Robert Tudor, M.Sc., 59 Portchester Road, Bournemouth (*transfer from Student Membership*).
Jones, Jacqueline H., B.A., F.R.G.S., 'The Warren,' The Ridge, Heswall, Wirral, Cheshire.
Paye, William Henry, 38 Millais Road, Dover, Kent.
Reynolds, Philip, 273 Knighton Church Road, Leicester.
Rogers, Ruth Helen, M.Sc., 23 Edge Lane, Chorlton-cum-Hardy, Manchester 21 (*transfer from Student Membership*).
Smith, Leslie Garrett, M.A., Ph.D., Institute of Geophysics, University of California, Los Angeles 24, California, U.S.A. (*transfer from Student Membership*).
Starling, David James, 15 Dalegarth Gardens, Purley, Surrey.
Wilkins, Flt. Lt. Norman Edward, D.F.C., 5 Redstone Park, Redhill, Surrey.

Foreign Members

Gates, W. Lawrence, 223 Beacon Street, Boston, Mass., U.S.A.
Giao, Antonio, 107 Rue Lauriston, Paris (16^{e}), France.
Kadmon, Naftali, Director of Training, Aero Club of Israel, 6 Hama'alot Street, Jerusalem, Israel.
Kaplan, Lewis David, Ph.D., U.S. Weather Bureau, Washington 25, D.C., U.S.A.

Student Member

Hodgkiss, Peter Keith, 61 Griffins Brook Lane, Bournville, Birmingham 30.

The following candidates were then declared to have been elected that day by the Council :

Fellows

Archbold, John William, M.A., 47 Buckingham Way, Wallington, Surrey.
Bell, Basil Edward, B.A., 11 Cambridge Park Court, Twickenham, Middlesex (*transfer from Student Membership*).
Blanchard, Paul Harwood, M.A., Red Gables, Beech Avenue, Sanderstead, Surrey.
Brownlow, Flt. Lt. Bertrand, Officers' Mess, R.A.F., Bawtry, Yorks.
Dixon, Roy, 62 Medway Parade, Perivale, Middlesex.
Evans, Dr. Daniel Marcus, M.R.C.S., L.R.C.P., B.Sc., Cartre, Ffestiniog, Merioneth.
Houghton, Instr. Lt. David Maurice, R.N., 21 Pendyffryn Road, Rhyl, Flints (*transfer from Student Membership*).
Kelly, William Lamont, 119 Wilson Street, Bulawayo, Southern Rhodesia (*transfer from Student Membership*).
Pearce, Alfred George, 24 Inman Road, Liverpool 21.
Procter, Dorothy Mary, 36A Westfield Road, Barton-on-Humber, Lincs. (*transfer from Student Membership*).
Storry, First Officer Colin Claud, c/o Qantas Empire Airways, Shell House, Sydney, N.S.W., Australia.

Foreign Members

Friedman, Don Gene, 87 Westgate, Cambridge, Mass., U.S.A.
Takeda, Prof. Kyoiti, Faculty of Agriculture, Kyushu University, Hakozaki-cho, Fukuoka, Japan.

Student Members

Gibson, George Geoffrey, ' Carisbrooke,' Southwell Road, Farnsfield, Nr. Newark, Notts.
Hughes, Wendy, 79 Chiswick Village, London, W.4.
Musk, David Rolin, 1 Market Street, Aylesbury, Bucks.

The following papers were read and discussed :

(a) ' Condensation nuclei and the development of radiation fog,' by A. C. Best (*Q.J.*, **79**, p. 112).

(b) ' Some further aspects of night cooling under clear skies,' by W. E. Saunders (*Q.J.*, **78**, p. 603).

The Earth; its origin, history and physical constitution. By Harold Jeffreys, F.R.S. Cambridge (University Press), 1952. 3rd Edition. Pp. xi, 392; 10 plates, 30 Figs. 70s.

In his opening remarks the reviewer of the first edition of this book in the *Quarterly Journal* in 1925 said ' it may give meteorologists something of a shock, on opening a book entitled *The Earth*, to find so little therein about the atmosphere.' The ' little,' in fact, amounted mainly to a brief account of ' Theories of climatic variation,' which the author included as an appendix, explaining that this topic, being a meteorological one, was outside the scope of the book. In the third edition of the book even this appendix has disappeared and so far as the meteorologist is concerned the cupboard is now, to all intents and purposes, bare. Apparently Prof. Jeffreys decided to omit climatic variation because he is now less hopeful of the adequacy of the methods of investigation and ' is inclined to think that ancient climates are more likely to tell us something about meteorology than vice versa.' Several other large branches of geophysics, such as terrestrial magnetism and atmospheric electricity are also omitted.

The second edition of *The Earth* was published in 1929 and, as might be expected from the many advances in geophysics during the last 23 years, considerable changes have been made in the present edition. The main change is the deletion of four chapters relating to the origin of the solar system, but the remaining chapters have been almost completely re-written and considerably enlarged. The first third of the book is mainly concerned with the elastic properties of the earth and with observational seismology, the next with geodesy and the last part with the earth's origin and history. These subjects are dealt with in a masterly manner; the treatment is theoretical rather than descriptive and Prof. Jeffreys generally includes a critical study of the work and theories he discusses.

Meteorologists who have been struggling to understand the problems of atmospheric movements may find consolation in some views expressed by Prof. Jeffreys after discussing evidence for the theory of continental drift. He thinks that turbulence in fluid motion, even in its simpler cases – and the atmosphere is far from being one of these – is comparable in difficulty with nuclear physics. He reminds us that the problem of predicting the path of a cyclone in temperate latitudes is similar to, though more complex than, that of predicting the motion of an individual eddy in a turbulent wind channel. One might infer that it was the present inadequate state of meteorological knowledge that induced Prof. Jeffreys to exclude meteorological topics from the scope of his book.

In a note on statistical methods at the end of the new edition, Prof. Jeffreys expresses astonishment that experimenters will spend months in making a series of observations and grudge the day or so needed to present the results in an intelligible form. The reviewer feels that ' day or so ' is, perhaps, an understatement and that an experimenter who has spent months on his observations should be prepared to spend a good deal longer than a day or so in making his results presentable. Prof. Jeffreys also complains that no more than one estimate in five is accompanied by a proper estimate of uncertainty; he considers that the so-called probable error should be abandoned in favour of the standard error.

A new feature in the third edition is that at the end of the book, in addition to the index, there is a very comprehensive bibliography based on the Royal Society system, in which the date of a paper is made second in importance only to the author's name. But since the index now covers subjects only and not authors' names there is no means of referring quickly to pages where a particular author is quoted. This is a very small point but it is mentioned since Prof. Jeffreys goes to some trouble in his preface to the new edition to explain the advantages of his bibliographical system.

The production of the book is of the usual high standard of the Cambridge University Press and although the price is high (roughly three times that of the previous edition, after allowing for the increase in length) this new edition can be strongly recommended as an authoritative and up-to-date treatise on the main branches of the physics of the earth.

F.J.S.

Die Ionosphäre. By K. Rawer. Groningen (Noordhoff), 1953. Pp. 189; 67 Figs., 7 tables. N.p.

The study of the ionosphere involves a series of problems, for a proper understanding of which an unusually wide knowledge is required. The topics include, for example, the magneto-ionic theory of wave-propagation with its explanation of absorption, refraction and reflexion in rapidly and slowly-varying media; the processes of production and disappearance of electrons in the layers, including a considerable amount of photo-chemistry; the nature of the ionizing radiations from the sun during normal and abnormal periods; the world-wide results of ionospheric soundings at different times of the day, year, and sunspot-cycle; the movements of the ionized layers under the influence of tidal forces and the earth's magnetic field; the interpretation of the results of investigations in geomagnetism and the auroral emission; and the effects of the ionosphere on the propagation of commercial radio waves, including forecasting of these effects. There are not many investigators who have a detailed grasp of more than a few of these complicated aspects of the subject. The unfortunate result is that those who might write valuable text-books on the ionosphere are often inhibited by the mass of material which they feel they could not do justice to. This lack of text-books in so important and complicated a subject is a serious disadvantage to all investigators.

The book *Die Ionosphäre* in which the well-known investigator K. Rawer surveys the whole subject, is therefore particularly acceptable just at this time. The topics mentioned above are all dealt with in a concise

and easily understandable way. In a book of less than 200 pages, covering such a wide subject, one could not expect a critical account of all the varied suggestions about ionospheric theory which have been put forward from time to time. The author has not attempted an account of this kind, instead he has presented simply and concisely one view of the subject; it is not necessarily everyone's view, some would be inclined to disagree with some of the statements made. The book is, however, of great value : it is this or nothing; the subject is in such a state that a fully critical account would be almost unreadable because so few conclusions would be reached with certainty. It is better to have a simple straightforward account of the subject, even if it is not accepted by all, rather than no account at all.

There has been a tendency, emphasized during the war, for work on the ionosphere to be concentrated in a few separate teams who have developed their own views on the subject, and it has not always happened that these have paid sufficient attention to the work of the others. In particular, it is probable that the English-speaking workers have tended to neglect the outstanding work of the German investigators, and it will therefore be particularly valuable to them that this book is written largely from the point of view of that mixed team of German and French workers which constitutes the Service de Prévision Ionosphérique Marine (S.P.I.M.) at Freiburg, in the French Zone of Germany. Through it workers in other groups will obtain a valuable insight into the views of the workers at S.P.I.M. and, if they are like the reviewer, they will be introduced to some important papers which they had previously overlooked.

The book will be most valuable in introducing new workers to the subject, provided they are warned that in many places the views expressed are not the only ones. More experienced workers will welcome it as an indication of the views of one active set of investigators, and if they disagree in places it will make them decide just why they disagree. It should certainly be read by all ionosphere workers. The reviewer, who is no German scholar, found the German simple, direct, and easy to read; its simplicity will be of considerable advantage to those, like him, who find involved German confusing.

J. A. Ratcliffe

Plant Environment and the Grower. By Sidney A. Searle. London (C. F. Casella and Co., Ltd.), 1952. Pp. v, 50; 26 Figs. 5s.

This booklet, addressed primarily to the technically minded horticulturalist, appears to be a successful attempt to elucidate some of the complex interrelations between the physical environment (particularly in the commercial glasshouse) and the growth of plants.

After dealing with soil and air temperature, there follow two sections on humidity and atmospheric moisture. A short account of sunshine and light leads to a consideration of transpiration, and the second half of the pamphlet is devoted to a discussion of soil water and its movement, and includes an account of the tensiometer and its use as a control instrument in irrigation.

Throughout there is explicit reference to the diurnal variation of meteorological elements (to which too little attention is paid in most text-books on horticulture). Some advice is given on the vitally important practical problems of measurement and instrumentation facing the intensive grower, and the treatment of this topic could, with advantage to the industry, be expanded.

Within the limits of the pamphlet there appears only one serious meteorological lapse, viz., the omission on p. 20 of explicit reference to the soil as a source of heat at night. It might also be mentioned that the figures of transpiration referred to on p. 25 were prepared by the Meteorological Office.

In short, an attractive work, which is of considerable interest to meteorologists as well as to horticulturists.

R.W.G.

Numerical Analysis. By D. R. Hartree, F.R.S. Oxford (Clarendon Press), 1952. Pp. xiv, 287; 31 Figs. 30s.

In recent years it has become apparent that the processes of numerical analysis can be profitably applied to several branches of meteorological research. Research into weather prediction by numerical process, first attempted by L. F. Richardson, has been given a new lease of life by the development of high-speed electronic computing machines and much work has been done both in this country and in America in order to obtain numerical solutions to various approximations of the equations of motion, continuity and state when applied to particular synoptic situations. Numerical methods of solving ordinary differential equations are being used by research workers in cloud physics and also by meteorologists who are interested in the study of standing waves. Consequently there has been a serious need for a text-book on practical mathematics, and the book under review goes a long way towards meeting this need.

The title of the book is a little misleading for one might think that this is a book on statistics. This is not the case, for the book deals entirely with the science and art of computation. It assumes no previous knowledge of the subject, limits analytic and algebraic work as much as possible and throughout the work places the emphasis on the practical aspects of the problem. It is quite evident that the author has had considerable practical experience in computation and the need for adequate checking is stressed. The book covers a wide range of topics : evaluation of formulae, finite differences, solution of simultaneous linear algebraic equations, solution of ordinary and partial differential equations, and organization of calculations for an automatic machine are perhaps those of most importance to meteorologists.

The first two chapters deal with the general aspects of computation. The use of desk machines is described at length, as is the organization of a calculation and the arrangement of work. Much useful information about unavoidable errors and the degree of accuracy of a computation can also be found here, as well as some comments about the ways in which random mistakes occur, how they can be detected, and how they can be put right.

The next two chapters on the evaluation of formulae and the use of finite differences are very well written with some useful worked examples, but these will be of only slight interest to the meteorologist. Random mistakes are introduced into the calculations and the way in which they can be found and corrected is very enlightening.

The chapter on ordinary differential equations shows how numerical solutions to various types of these equations can be obtained. One-point and two-point boundary conditions are discussed. The methods described in detail are the step-by-step processes through a series of equal intervals of the independent variable, Richardson's deferred approach to the limit, and various iterative processes.

The chapter on partial differential equations is of great interest to the meteorologist engaged in research into the use of numerical methods in weather prediction, for quite a large part of this chapter is devoted to methods of solving the Laplace, Poisson and Helmholtz types of equations, which are equations which occur frequently in dynamical meteorology. The relaxation method is the most suitable for hand computation and is discussed at length but there is also a short section dealing with the Richardson – Liebmann process which is more suitable than relaxation methods for use on electronic computing machines.

The final chapter of the book gives an introduction to the problem of organizing a calculation for EDSAC, the electronic computing machine at the Cambridge mathematical laboratory. It is written in non-technical language and is very easy to follow. Two elementary examples of arranging a set of instructions which whould enable the machine to carry out a simple calculation are given.

The book also deals quite adequately with methods of solving non-linear algebraic equations, interpolation, quadrature and differentiation, and these methods may from time to time be needed by meteorologists.

The book is written very well indeed and is easy to read. There is little literature on the subject to compare it with, but it is outstanding because it emphasizes the practical problems of computation. The aim of the book is to provide practical information about what calculation to make to obtain the required result, and how to make it. It does this, and is thoroughly recommended.

F.H.B.

Propagation of Short Radio Waves. Vol. 13, *M.I.T. Radiation Laboratory Series.* Edited by D. E. Kerr. New York (McGraw-Hill), 1951. Pp. xvii, 728; 299 Figs., 47 tables. 85s.

The use of radar at sea. Edited by Capt. F. J. Wylie, R.N. London (Hollis and Carter), 1952. Pp. xv, 279; 170 Figs., 9 tables. 30s.

A considerable amount of research on the propagation of short radio waves was carried out during the second world war. Many of the results of this research have been published in the form of reports with only a limited circulation. The purpose of the first of these two books is to summarize the state of knowledge, regarding propagation in the wave band from about 3 m to 1 mm, at the end of the war, though footnotes indicate that reports as recent as 1950 have been considered. It is written by members of the Propagation Group of the Laboratory as an interim report for research workers and covers work done in America, England, Canada and other Dominions. Although not a text-book each chapter gives sufficient of the basic background of its subject for an appreciation of the work described.

After a brief introductory chapter, the second chapter of about 150 pages deals with the theory of propagation in a horizontally-stratified atmosphere, including the computation of field strength with a linear modified-refractive-index profile. Chap. 3 considers in some detail the meteorological parameters which govern the profile of refractive index up to about 2,000ft and includes a discussion of the effect of eddy diffusion and of heating and cooling from below with numerous empirical results. The fourth chapter summarizes the principal experiments on the investigation of the effects of atmospheric refraction on microwave transmission. Chap. 5 deals with the effects of reflection of radio waves from the earth's surface. A long Chap. 6 discusses the radar cross-section of a number of targets, sea echo, echo fluctuations and clutter echoes. The seventh chapter is devoted to meteorological echoes and includes both theoretical work and a number of photographs of typical P.P.I. displays. Finally the last chapter gives a theoretical and empirical treatment of attenuation owing to absorption by gases, water vapour, liquid-and solid-precipitation particles. Although the preface disclaims any attempt to deal with the forecasting of radio and radar-propagation performance or with climatological studies there is no doubt the meteorological forecaster who has to deal with such matters would find some assistance, particularly in Chaps. 3 and 7. For the research worker the book can be thoroughly recommended. The production, of course, is up to the usual McGraw-Hill standard.

The second of these two books is also a joint work by a number of authors but is written for a very different reader. It is produced by the Institute of Navigation with the declared aim 'to give the mariner a complete and practical guide to the applications of marine radar and to the many factors which affect the results obtainable.' The first three chapters deal with the general principles of radar and the equipment, and the next four with the propagation of waves, radar meteorology, interpretation of echoes and unwanted

echoes. There follow four chapters related to radar as a navigational aid and six chapters dealing with miscellaneous matters, including maintenance. The treatment throughout is simple and involves no mathematics. The text is plentifully illustrated with diagrams and the photographs of radar displays are good. Only the mariner can decide whether the book achieves its aim and the reviewer claims no special knowledge of the sailors' problems. Nevertheless it is apparent that the contributors to the book are practical men with appreciation of marine problems.

A.C.B.

The tropical rain forest. By P.W. Richards. Cambridge University Press, 1952. Pp. xviii, 450; 43 Figs., 15 plates, 39 tables. 63s.

The tropics cover nearly five times the area of the arctic and antarctic regions combined; they support a vast population, and contain almost limitless natural resources. An extension of our knowledge of tropical regions is of first importance, but botanists, no less than meteorologists, have hitherto been inclined to neglect the tropics in favour of temperate regions. Prof. Richards's book is a detailed study of the ecology of tropical rain forests, well written, well produced, and very fully documented. The line drawings are excellent; the photographs good considering the grey twilight which prevails within the forest.

The author treats the forest as a plant community, whose members are influenced by one another and by environmental factors such as climate, topography and soil. Little is said, on the other hand, about the influence of the forest on its environment, the animal life which inhabits it, or its economic value to man. For this reason the book will be of value mainly to botanists and foresters, while the zoologists, geographers and others, to whom also the author addresses his work, will find it somewhat heavy going, particularly as no glossary of botanical terms is included.

Meteorologists will be interested in the section dealing with environment, which includes a masterly discussion of the available information regarding the climate of the rain forest regions and the microclimate within the forest. The climatic requirements are a high and very even temperature, and heavy rainfall spread over the greater part of the year; typical rain forests are not found where there is a prolonged seasonal drought. The areas covered by tropical rain forests (which, incidentally, account for about half the forested areas of the world) are irregularly distributed, here and there extending beyond the tropics, and in other places to only a few degrees north or south of the equator. This irregularity is attributed mainly to the uneven distribution of land and sea and its effect on rainfall. In tropical Australia, for instance, the rain forest in confined to a strip along the Queensland coast which is exposed to the S.E. Trades.

The author uses saturation deficit as an index of atmospheric humidity, for this gives a measure of the evaporating power of the air. In the interior of the rain forest the air remains nearly saturated throughout the twenty-four hours, particularly in the wet season, and it is remarkable that many of the lesser plants and trees within the forest show such rapid growth where the rate of transpiration must be slow, and the light is poor.

Owing to erosion and leaching by heavy rains, the soil in open situations in the tropics is often poor. Once the rain forest has become established, however, it protects the soil; the plant nutrients are locked up in the living vegetation and in the humus layer, and luxuriant growth is possible. Man, when he clears the forest, destroys this protection ; after a very few years of cultivation the soil becomes impoverished, the site is abandoned, and a fresh clearing is made. This wasteful system of shifting cultivation is practised by the natives in all rain forest regions.

The author deplores the rapid destruction of tropical rain forest by man. We agree that the timber resources of the forest could be more effectively utilized, and the cleared land more economically cultivated. As the author suggests, large areas of virgin forests in different parts of the tropics should be conserved on account of their botanical and scientific interest. Personally we do not agree that the rain forest has much aesthetic value. There is little beauty in those tall unbranched trunks rising to a dense canopy of dull green above, the tangle of climbers and ' stranglers ' beneath, the uniformity of the leaves, and the absence of seasonal changes. What can compare with a beech tree in spring or a glade carpeted with bluebells and primroses ?

The world is running short of food, and will soon run short of fuel. Tropical lands, where sunshine and rainfall are bountiful, can surely be made to produce more food, if the problems of soil conservation can be solved. And perhaps, as Hoyle suggests, our future supply of energy will not be from atomic sources, but through the capture of solar radiation by suitable tropical vegetation, which can be made to yield fuel alcohol. ' The jungle is neutral ' ; maybe it can be replaced by plant life which is beneficial to mankind.

G.S.P.H.

Bulletin de l'Observatoire du Puy de Dôme (Quarterly). 2nd Series, No. 1; Jan. 1953. University of Clermont-Ferrand. Pp. (9½in. × 6in.) 32; Figs. 100 fr.

Geofisica e Meteorologia. Vol. I, No. 1, 1953. Bulletin of the Italian Society for Geophysics and Meteorology; Genoa. Pp. (12in. × 8in.) 28; Figs. N.p.

We welcome warmly the reappearance, after a lapse of some years and in a slightly modified form, of the *Bulletin of the Puy de Dôme Observatory.* Its editor is H. Dessens; its aim, stated on the cover, to act

as a means of liaison between physicists engaged in the study of clouds and of means of influencing their development. E. M. Fournier d'Albe and A. M. A. Lateef report the results of an investigation of large hygroscopic particles in suspension in the atmosphere of Western Pakistan. C. Lafargue discusses the microphysics of supercooled clouds and atmospheric freezing nuclei, while H. Dessens contributes an interesting brief article on the appearance of ice particles in cumulus. Short reports on cloud-seeding experiments in Tunisia and Algeria, by J. Tixeront and R. Serpolay respectively, complete the slim volume. Cloud physicists, into whose hands comes this first number, will find not only that there is nothing they dare skip, but that they will want to read it from cover to cover. The bulletin is clearly printed on good-quality paper.

We greet also a new Italian journal. This is excellently produced; it carries on its cover a superb photograph of a waterspout. The first number contains several meteorological articles; in particular, I. Ranzi describes an expedition to Deception Island, in the Antarctic, for ionospheric investigations; L. S. Mortola writes on cloud microphysics and artificial rain-making, and H. Israel discusses relations between electrical and meteorological phenomena. The articles are all brief and, with the exception of that by Israel, comparatively non-technical.

T.W.W.

Obituary Notices

Major H. C. GUNTON, M.B.E., F.R.Ent.S.

A former Vice-President and Director of Phenological Studies has been lost to the Society by the death on 5 February 1953, after a short illness, of Herbert Charles Gunton. Born at Manchester on 11 April 1874, Gunton received his education and training in that city, taking first a course in general engineering at the Technical School and later specializing in electrical technology at Owens College. Having completed his apprenticeship at the works of Laurence Scott & Co., Norwich, and after a tour of the United States to inspect power, traction and lighting undertakings, he came to London. In 1897-98 he was electrical engineer in charge of testing engines and generators for the firm of Peter Brotherhood and in 1898 he joined the staff of Kennedy and Jenkin, under whom he served as resident engineer in turn to the Dublin Corporation, the Great Northern Railway of Ireland, the Manchester Corporation, and the Great Western Railway. In 1908 he undertook inspection duties at home and abroad on behalf of the Crown Agents for the Colonies and in 1909 was appointed principal power engineer to the Post Office, in which capacity he served on various departmental committees and did much other important work. Soon after the outbreak of war in 1914 Gunton became engineering staff officer to General Sir O'Moore Creagh, V.C. In 1917 he went to France with the rank of major to study and advise upon signal organization under conditions of open warfare. For his services there he received the award of M.B.E. (Military Division) in 1919. After his return to civilian life he again represented the Post Office on numerous committees and panels. During the next few years his high technical and organizing abilities achieved wide recognition outside as well as inside the sphere of his professional activity. Among the matters on which his counsel was enlisted in the national interest were the lighting of public buildings, the standardization of electrical machinery and fittings, the design of mechanical stamping appliances and sorting apparatus, and the simplification of Post Office forms. In 1929 he acted as consulting engineer to the Racecourse Betting Control Board during the evolution of the electric totalisator. A little earlier he had attended, as one of the British delegates, a meeting at Geneva of the Advisory Committee of the International Electro-technical Commission on the rating of electrical machinery. In 1934 Gunton reached the scheduled age for retirement from the Civil Service, but such was the value set on his services by the Post Office that arrangements were made to retain them in an advisory capacity for a further twelve months.

Nature study had been Gunton's principal hobby from boyhood. He specialized in entomology and was particularly interested in the British lepidoptera, of which he accumulated a notable collection. The effects of climate and weather on insect life had long claimed his attention and formed the subject of his first communication to the Entomological Society of London, in 1920. After his retirement from the Civil Service he concentrated increasingly on bioclimatological problems. In 1935 he read a paper before the Society on ' Phenological records of British lepidoptera ' and in the following year he was elected to our Fellowship. At that time responsibility for the guidance of the Society's phenological investigations (maintained continuously since 1874) was in the capable hands of Mr. I. D. Margary, but other calls were necessitating Mr. Margary's early relinquishment of these duties. Gunton, invited by the Council to fill the vacancy, set about his task with characteristic energy. The work was to a large extent reorganized on a scheme of his own devising which placed both the observational and analytical routines on a sounder scientific basis than could hitherto have been claimed for them. Eleven consecutive issues of the *Phenological Report*, covering the years 1937 to 1947, were produced for the Society by Major Gunton, each one as exhaustive as the available space would permit, and the entire series constituting a model of patient, painstaking endeavour. He was a firm believer in the advantage of letting observations tell their own story graphically; to this end he introduced an ingenious group of diagrams which condensed a vast amount of information into a small compass. At first sight these appeared to be of formidable complexity (one critic described them as ' graphs *in excelsis* '), but actually it needed no great effort to master them and to appreciate the vivid picture which they gave of biological responses to weather conditions through each season. An explanation of his methods and procedure was included in his book *Nature study above and below the surface*, published through H. F. & G. Witherby in 1938.

From 1941 to 1946 Gunton carried on the phenological studies with difficulty, for he was then serving as temporary assistant electrical engineer to a production branch of the Admiralty and was stationed at Bath; the bulk of his work for the Society had to be done by night or during short intervals of leave. Nevertheless, he succeeded in getting each issue of the *Phenological Report* ready for publication and discussion by the scheduled date. It was mainly at his instance that, in 1945, the Royal Entomological Society and, soon after, the British Ecological Society joined forces with our Society to form a bioclimatic committee whose aim was to foster research and education in bioclimatology. As a result of this committee's activities the phenological investigations maintained for so long by our Society became much better known and more widely appreciated. There were many calls on Gunton's services for co-operation, lectures and advice, some of them coming from abroad. But within the Society itself the appeal of phenology had by now waned to such an extent that attendance at the annual meetings held to receive and discuss Gunton's reports was reduced to less than one per cent of the membership year after year. In 1947 Gunton expressed his intention of resigning the directorship with effect from 1948, and while the future of the work was under consideration a suggestion came before the Council that an association of meteorologists was not the best possible authority to conduct a modern phenological survey. This view having been accepted, it was decided that the *Phenological Report* for 1947 should be the last to appear under the Society's auspices. Collection of the observations was continued for a few years in the hope that some biologically qualified body might take over their analysis and publication; but this did not happen, and in 1951 the organization was disbanded. Thus withered and fell

a branch of the Society's activities which had begun growth in the year of Gunton's birth and had been brought by him to its fullest fruitage during his seventh and eighth decades. In 1948 the Council signified their appreciation of Major Gunton's services by selecting him as one of the first two recipients of the Darton Prize – a newly instituted award made in respect of 'the most useful digest of meteorological observations or the results of recent research.'

At the funeral service for Major Gunton in the church of Chalfont St. Peter, Buckinghamshire, on 9 February 1953 the Society was represented by Mr. E. L. Hawke.

E.L.H.

Flying Officer C. G. KEYTE, D.F.C.

Flying Officer Cyril Geoffrey Keyte, who had held Fellowship in this Society since 1949, was killed at the age of 33 on the night of 26 September 1952 while undertaking an instructional flight for the R.A.F. in South Africa. He saw much active service with Bomber Command from 1943 to 1945 and was awarded the D.F.C. for conspicuously courageous and successful performance of duty on an operational tour. After the conclusion of the war he transferred to B.O.A.C., whose flying-boats he repeatedly took to distant parts of the world during the next few years. Early in 1951 he responded to an appeal from the R.A.F. for the help of experienced pilots in training young airmen to the use of bombing and fighting craft. It was while thus engaged on his country's behalf that he met his death. Flying Officer Keyte had well over 5,000 hours of time in the air to his credit.

E.L.H.

Mr. C. C. LOWATER

Mr. Charles Cyril Lowater, well known in Nottinghamshire as a leading all-round sportsman and as head of an important firm of wholesale merchants founded by his father, died suddenly at his home, Bramcote House, Burton Joyce, on 17 September 1952 in his 53rd year. He had played hockey for the county and was chairman of the Nottinghamshire Cricket Club selection committee in 1951. During the war of 1939-45 he served in the Royal Army Pay Corps. Mr. Lowater was elected a Fellow of this Society in 1939.

E.L.H.

Corrigendum to January 1953 issue

T. W. Wormell, p. 38, References—

Smith, L. G. 1951b *Quart. J. R. Met. Soc.*, **77**, p. 525.

For p. 525, *read* p. 683.

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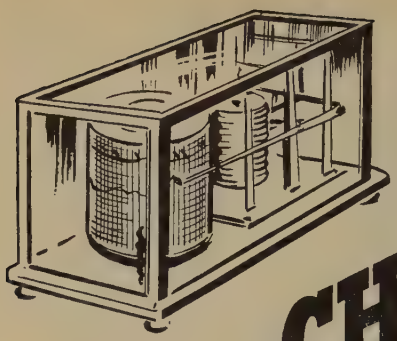

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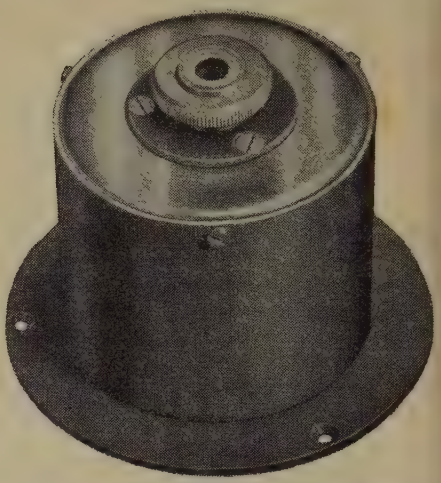

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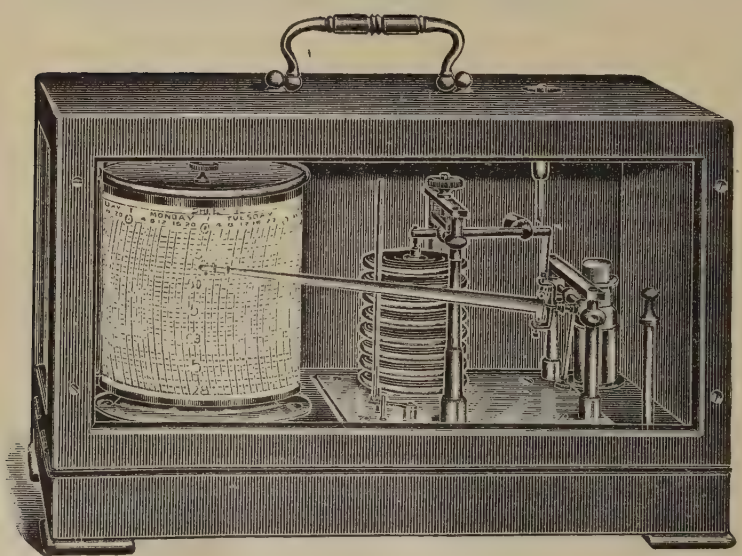

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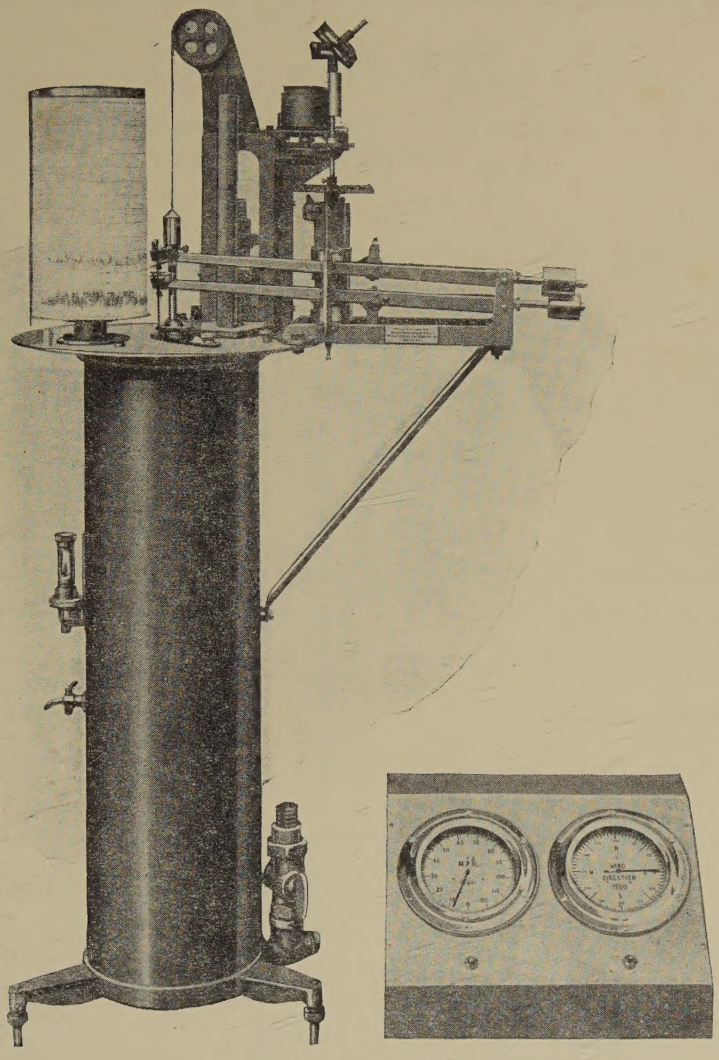

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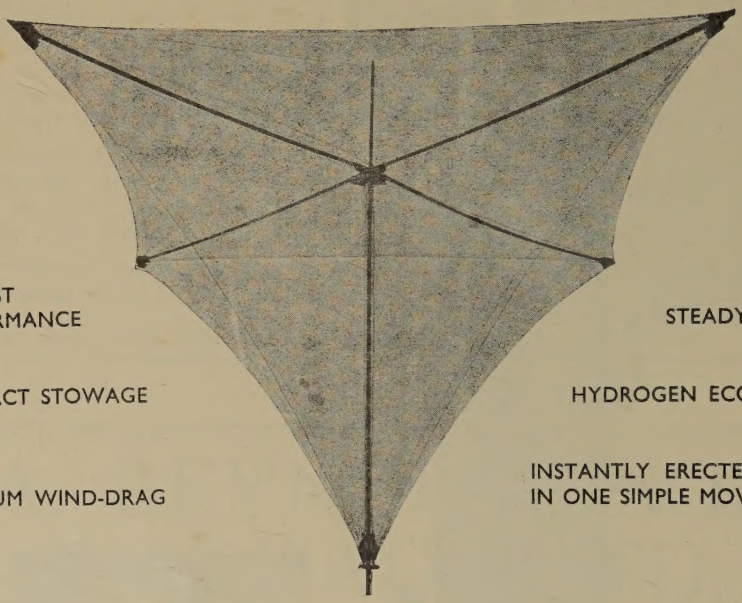

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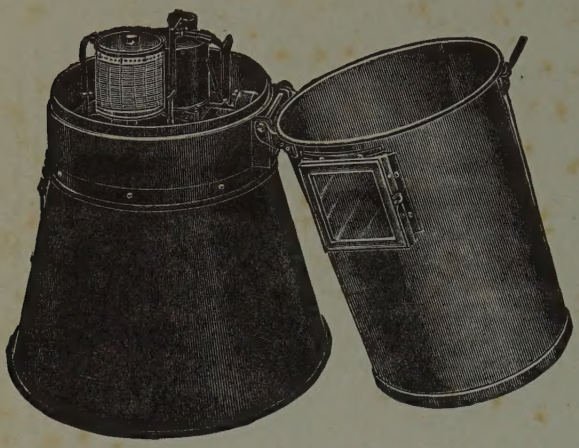

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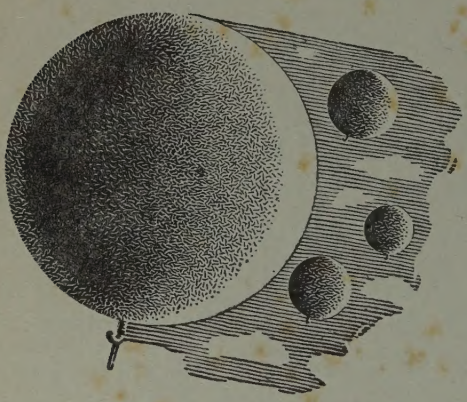

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